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Local soldier fatalities and war profiteers: New tests of the political cost hypothesis[☆]

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A B S T R A C T

We test the political cost hypothesis using local soldier fatalities as a source of as-if-random variation in the threat of political costs for local defense firms. Soldier fatalities vary the threat of political costs for defense firms because the U.S. tradition of shared sacrifice during war vulgarizes war profits amid dead soldiers. Local defense firms record more income-decreasing accruals, equal to 1.17 percent of total assets, in response to a one standard deviation increase in local soldier fatalities (an additional 29 soldier fatalities in the average state-year). A wide variety of robustness tests corroborate our inferences.

1. Introduction

Political costs, such as excess-profit taxes and price controls, are government-imposed transfers of wealth from the private sector. The political cost hypothesis predicts that firms will exploit discretion in accounting policies to orchestrate the appearance of lower profits in response to an increase in the threat of these political costs (Watts and Zimmerman, 1978, 1986). Several prior studies examine associations between the threat of political costs and accounting choices and find

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support for the political cost hypothesis.[1] The purpose of this paper is to overcome the key obstacle to causal inference in this prior literature: that firms' susceptibility to political costs is often endogenous to its policies (Demski, 1988, p. 625; Watts and Zimmerman, 1990, p. 131). We advance the political cost literature with empirical tests examining discretionary accounting choices in response to a strong source of variation in the threat of political costs that affects firms randomly across time and place. This research design permits us to draw credible causal inferences regarding the effect of the threat of political costs on firms' accounting choices.

We use 6418 local American soldier fatalities during the wars in Afghanistan and Iraq between 2003 and 2012 as an as-if-random source of local variation in the threat of political costs for defense firms. Soldier fatalities vary the threat of political costs for defense firms because of the U.S. tradition of shared sacrifice during war (Bank et al., 2008; Feldman and Slemrod, 2009). The U.S. tradition of shared sacrifice vulgarizes war profits amid soldier fatalities. Local soldier fatalities increase public awareness of the substantial human costs of war,[2] and the juxtaposition of dead local soldiers and war profits catalyzes public opinion against war profits because it highlights the uneven and inequitable distribution of the costs of war. In a synopsis of moral exhortations against war profits throughout U.S. history by public figures, Brandes (1997, p. 242) concludes that “[t]he basic ethical assumption supporting the limitation of war profits [is] that, while others [are] sacrificing their lives, no citizen should rise economically as a consequence of misfortune.”

Soldier fatalities vary the threat of political costs for defense firms because they increase public opposition to war profits. Public opposition to war profits is a source of political costs for defense firms because political costs are determined by public policy, and public policy is determined by politicians accountable to democratic institutions such as “public opinion, public debate, rallies and protests” (Avant and Sigelman, 2010, p. 236). The American public has a long history of opposing defense firm profits, and, accordingly, the government has a long history of imposing political costs on the defense industry (Stigler and Friedland, 1971; Bank et al., 2008). The U.S. government has imposed these costs through excess-profit taxes, profit restrictions, price controls, industry-wide civilian audits of military contracting and industry-wide contract renegotiations during the War of Independence, the Civil War, World War I, World War II and the Korean War (Brandes, 1997). In addition, Congressional representatives introduced legislation threatening to impose political costs on war profiteers four times during our sample period.

We use the permanent residence of American soldiers who perish in Afghanistan or Iraq to identify as-if-random state and year variation in the threat of political costs for defense firms. We use local soldier fatalities as a source of variation in the threat of political costs because local soldier fatalities offer research design advantages that support causal inference. First, local soldier fatalities lead to strong revisions in local public opinion about the substantial human costs of war. Political consequences of local soldier fatalities include increased opposition to war and higher voter turnout (Althaus et al., 2012; Kriner and Shen, 2013; Koch and Nicholson, 2016). Second, after controlling for state fixed effects, the state of permanent residence and the year of death, soldier fatalities are random across time and place and so are not individually, much less jointly, predictable.[3] Consequently, firms cannot change their headquarter location in anticipation of political costs spurred by future local soldier fatalities. Third, local soldier fatalities are exogenous to the firm. Local defense firms cannot influence the permanent residence of a deceased soldier or the timing of a soldier fatality. Fourth, we know of no economic mechanism relating local soldier fatalities to local defense firms' discretionary accruals. For example, we find no relationship between local soldier fatalities and local defense firms' procurement revenues. Orthogonality between discretionary accruals and the direct economic effects of variation in the threat of political costs allows us to isolate local defense firms’ discretionary accrual-based response to increases in the threat of political costs, thereby permitting well-identified tests of the political cost hypothesis.

The political cost hypothesis predicts that defense firms local to soldier fatalities will respond to increases in the threat of political costs by using accruals to orchestrate the appearance of lower profits. Our tests of this prediction control for an exhaustive list of known accrual determinants and include either state and industry $\times$ year or firm and industry $\times$ year fixed effects. We find that local defense firms’ income-decreasing discretionary accruals increase with the state-year per capita number of local soldiers who perish in Iraq or Afghanistan during the firm-year.[4] The economic magnitude of the effect is large; a one standard deviation increase in local soldier fatalities from the state-mean, equivalent to approximately 29 additional local soldier fatalities in the average state-year, is associated with income-decreasing discretionary accruals equal to 1.17 percent of total assets.

We next test for a predictable reversal of income-decreasing accruals. Althaus et al. (2012), Kriner and Shen (2013), and Koch and Nicholson (2016) document that the effect of local soldier fatalities on political outcomes (e.g., voter sentiment, voter turnout, opposition to the war) dissipates within one year. Their findings inform our prediction of when the threat of

[1] See, e.g., Key (1997); Cahan et al. (1997); Han and Wang (1998); Navissi (1999); Monem (2003); Patten and Trompeter (2003); Johnston and Rock (2005); and Byard et al. (2007). Cahan (1993) is an exception. Other studies find a positive association between import relief investigations, which offer political benefits that increase with the appearance of economic injury, and accounting choices. See, e.g., Jones (1991); Magnan et al. (1999); Godsell et al. (2017).

[2] An example of increased visibility of the war following a local soldier fatality is the death of Major Brent Taylor, after which volunteers in his hometown hung a 400-pound flag across a local canyon as tribute (Deseret News, 2018).

[3] For example, deviations from the state-mean of soldier fatalities do not predict future deviations from the state-mean (see Appendix Table 1A).

[4] We draw the same inference when we instead use a raw count of soldier fatalities by state-year or an indicator variable equal to one when local soldier fatalities are one standard deviation above the state-mean and zero otherwise.

political costs will dissipate and when local defense firms will unravel income-decreasing accruals. We predict that the income-decreasing discretionary accruals recorded when local soldier fatalities are high in year t will reverse in $t + 1$. Adapting the research design prescribed by Dechow et al. (2012) to our setting, we find that local defense firms unravel income-decreasing discretionary accruals in the year subsequent to increases in local soldier fatalities. We find that local defense firms record income-increasing discretionary accruals equal to 0.52 percent of total assets in $t + 1$.

Our results vary predictably in the cross-section of local defense firms. We first find that our result strengthens with defense firms' vulnerability to political costs. Political costs, such as excess-profit taxes, are likely more costly for the firm when defense revenues, as a proportion of total sales, are larger. Accordingly, we find that the magnitude of income-decreasing discretionary accruals increases monotonically with the defense procurement sales ratio.

We next find that our result predictably strengthens with the visibility of local soldier fatalities. If local soldier fatalities spur local public attention to war costs and war profits, then the attention – and defense firms' response to public attention – should vary with local media coverage of soldier fatalities. We expect the relationship between local soldier fatalities and local defense firms' accounting choices to be stronger in states and years in which the local media provides more coverage of local soldier fatalities. We find that greater news coverage of local soldier fatalities strengthens the association between income-decreasing discretionary accruals and local soldier fatalities.

We next find that our result predictably weakens with defense firms' political strength. If defense firms with more market power vis-à-vis the government are politically stronger and less vulnerable to political costs, then the incentive to engage in earnings management to reduce political costs will be weaker. We test this expectation using two market-power proxies. First, we examine defense contract details to identify defense firms providing advanced weapon technologies to the U.S. Department of Defense (DOD). We expect these defense firms to be less vulnerable to political costs because the market for advanced weapons technologies is small, proprietary and oligopolistic vis-à-vis, for example, the more competitive market for textiles and foodstuffs where the DOD's vendor-switching costs are lower. High-market-power firms have a weaker incentive to engage in earnings management to reduce political costs because history shows that these defense firms can pass the political costs (e.g., excess-profit taxes) along to the government buyer through higher prices (McCartney and McCartney, 2015). Similarly, we examine defense firms that have more foreign sales. Having more foreign customers renders defense firms less reliant on sales to the U.S. DOD. These defense firms have a weaker incentive to engage in earnings management to reduce political costs because these defense firms can more easily exit the U.S. market if the government imposes onerous political costs on U.S. defense firms. Consistent with defense firms' market power weakening earnings management incentives, vendors of advanced weapon technologies and vendors with more foreign customers exhibit a weaker accrual response to variation in local soldier fatalities.

We conduct four placebo tests appropriate for our setting. These tests show that our effect 1) does not affect government procurement firms uninvolved in the defense industry, 2) does not manifest when soldiers have weak ties to the local community (using the state in which the soldier was based before deployment instead of their permanent residence state), 3) does not manifest for publicized non-soldier fatalities unrelated to war (police fatalities), and 4) does not manifest when soldier fatalities are unpublicized (veteran suicides).

Our inferences are robust to additional sensitivity tests. First, our results are insensitive to using the state in which defense firms have their largest operations rather than the state of their historical headquarters to identify when defense firms are local to soldier fatalities. Second, we find that our results are robust to removing the large number of defense firms headquartered in California or New York, or any state, from our sample. Third, our results are robust to removing recession years, or any year, from our sample.

Fourth, we supplement our main analyses using real actions-based earnings management models that examine the behavior of sales, general and administrative expenses, production costs, advertising expenses and research and development expenditures around state-year soldier fatalities. These real actions are an alternate, if costlier, method by which firms can orchestrate the appearance of lower profits (Badertscher, 2011). We find that local defense firms incur discretionary expenses around local soldier fatalities. Consistent with the distinction between accrual and real actions-based earnings management, we find no "reversal" of income-decreasing real actions in $t + 1$ (Ernstberger et al., 2017).

Three replications of the foregoing tests, reported in Online Appendices 1 to 3, complete our empirical analysis. We replicate tests using 1) a performance-matched sample, 2) an entropy-balanced sample, and 3) a defense firm-only sample. These tests address concerns regarding inherent differences between the defense and non-defense firms comprising our main sample. Replicating our tests using these alternate samples corroborates all inferences from our main tests.

Our causal evidence significantly advances the political cost literature because our research design addresses the key challenge when testing whether the threat of political costs determines accounting choices: firms' susceptibility to political costs is often endogenous to its policies (Demski, 1988, p. 625; Watts and Zimmerman, 1990, p. 131). We form causal inferences regarding the effect of variation in the threat of political costs on accounting decisions by exploiting a strong source of variation in the threat of political costs that randomly affects firms across time and place. Random variation in the threat of political costs addresses endogeneity concerns because variation in the threat of political costs can only be randomly associated with firm policies. This research design permits us to draw credible causal inferences regarding the effect of the threat of political costs on firms' accounting choices, and, overall, we find unambiguous support for the political cost hypothesis. Evidence-based policymaking requires causal evidence (Leuz and Wysocki, 2016), and our causal evidence, which corroborates non-causal inferences from the prior literature, increases the credibility of the entire body of political cost research for policymakers.

Our study is also the first to examine wartime earnings management. The war economy during our sample period is large with cost estimates ranging from $1.5 trillion (Department of Defense, 2019) to $5.2 trillion (Crawford, 2017). As we demonstrate in Section 2, policymakers expend significant effort and expense to curb war profiteering. Our results show that political cost theory describes defense firms' accounting choices, that these choices vary predictably with defense firms' political strength vis-à-vis the U.S. government, and that policymakers require an understanding of accruals to paint an accurate picture of defense firms' operating performance during war. Lastly, we contribute to the political science literature that has thus far documented associations between soldier fatalities and political outcomes (Althaus et al., 2012; Koch and Nicholson, 2016).

We organize this paper as follows. In Section 2, we describe the American history of public opposition to war profits and the long list of political costs historically imposed upon the defense industry. We describe the political cost literature in Section 3. We develop our hypothesis in Section 4. Section 5 describes our data. We discuss our research design in Section 6. Section 7 presents our results, and Section 8 concludes.

2. A history of political costs in the defense industry

The U.S. exemplifies a tradition of shared sacrifice during war. Bank et al. (2008, p. xiv) write, "[W]ars often create a new political atmosphere − one characterized by feelings of solidarity and shared sacrifice …. [T]axes are never popular, but they are never more popular than during wars." Reflective of the tradition of shared sacrifice is that, when the Revenue Bill of 1942 expanded the income tax base during World War II to include not only the wealthy and upper middle class but also millions of common laborers, 90% of Americans responded "yes" when asked whether the income taxes were fair. A tradition of shared sacrifice is also evident in that calls for new war taxes were loudest after the government imposed conscription (a tax on human capital [Bank et al., 2008]). Conscription exemptions offer another example. Exemptions from Civil War conscription, at a price, helped precipitate the deadliest riot in U.S. history (105 fatalities) because, by placing the human cost of war disproportionately on the poor who could not afford the exemption fee, exemptions violated demands for shared wartime sacrifice (McKay, 1991).

The U.S. tradition of shared wartime sacrifice has meant that public support for imposing political costs on war profiteers is pervasive throughout U.S. history. Public figures frequently direct moral exhortations toward the defense sector when at war. Early in U.S. history, George Washington was a "frequent and forceful opponent of war profits" (Brandes, 1997, p. 48), writing, "No punishment, in my opinion, is too great for the man who can build his greatness upon his country's ruin" (Lengel, 2008, p. 397). President Abraham Lincoln denounced war profiteers as "worse than traitors" during the Civil War. President Harding admonished war profits during World War I, declaring that "[t]here is something inherently wrong, something out of accord with the ideals of representative democracy when one portion of our citizenship turns its activities to private gain amid defensive war while another is fighting, sacrificing, or dying for national preservation" (Brandes, 1997, p. 180). President Franklin D. Roosevelt was a vociferous opponent of war profiteering. Leading up to World War II, President Roosevelt "realized that he could never hope to win his countrymen's support for rearmament if they believed that the sacrifices it would entail would be borne unfairly" (Brandes, 1997, p. 232). In directing Congress on war tax policy, Roosevelt stated that "excessive profits undermine unity and should be recaptured" (Bank et al., 2008, p. 95) and asked for legislation for the "prevention of profiteering and equalization of the burdens of possible war" (Brandes, 1997, p. 233).

At the outbreak of the Korean War, President Truman called for a tax program that would have "as a major aim the elimination of profiteering" (Bank et al., 2008, p. 113). President Nixon later stated that "there is no excuse or justification for … profits as a result of war while men are dying on the battlefield" (Bank et al., 2008, p. 114). Six months later, with the promulgation of the Excess Profit Taxes Act of 1950, President Truman introduced new excess profit taxes. In a synopsis of these moral exhortations against war profits throughout U.S. history, Brandes (1997, p. 242) concludes, "The basic ethical assumption supporting the limitation of war profits [is] that, while others [are] sacrificing their lives, no citizen should rise economically as a consequence of misfortune." Overall, the U.S. tradition of shared sacrifice during war vulgarizes war profits amid soldier fatalities. That is, the juxtaposition of dead soldiers and war profits catalyzes public opinion against war profiteers because it highlights the uneven and inequitable distribution of the costs of war; soldiers fighting in defense of the country are maimed and slaughtered in ditches abroad while war profiteers enrich themselves in comfort at home.[5]

The public opposition to war profits described above is a source of political costs for defense firms because political costs are determined by public policy, and public policy is determined by politicians accountable to democratic institutions such as "public opinion, public debate, rallies and protests" (Avant and Sigelman, 2010, p. 236). Accordingly, along with moral exhortations, the public and the government have imposed substantial costs on the defense industry. For example, as the U.S. mobilized for World War I in 1916, the Wilson administration proposed a special 12.5 percent tax on any income from munition sales that exceeded 8 percent of invested capital. Congress later adopted an excess-profits tax of 60 percent of any net income that exceeded 33 percent of invested capital (Brandes, 1997, p. 172), and the government re-introduced this tax during World War II.

[5] This sentiment, strongest in the 1930s as World War I veterans gained political power, led to the "merchants of death" theory that posited war merchants cultivated international tensions in their search for profit.

An example from the years between World War I and World War II is the Tobey amendment to the 1934 Vinson-Trammell Act, a navy shipbuilding bill that commissioned the manufacture of new warships and aircraft. In the congressional session in which the legislation passed the amendment, house representatives decried “excessive profits in naval and aircraft construction contracts,” and the final version of the amendment restricted naval and aeronautic firm profits to 10 percent of cost (Brandes, 1997, p. 228). A contingent of Congressmen would later attempt to expand the profit restriction to all munitions manufacturers (Brandes, 1997, p. 233).

Leading up to World War II, Franklin D. Roosevelt stated in 1940 that he did not “want to see a single war millionaire in the United States as a result of the war disaster” (Brandes, p. 233). His vision took shape through the aforementioned excess-profit taxes and a comprehensive system of price controls. Future President Harry S. Truman vigorously enforced these industry-wide price controls, and the Truman Committee prosecuted 33,036 firms for violations by the end of World War II (Brandes, 1997, p. 260). During the Korean War, excess-profit taxes were reintroduced, and the U.S. Renegotiation Board was created to guard against profiteering by defense contractors.[6]

Public outcry against profits occasionally manifests as anti-war profiteering protests. A first example includes an October 3, 2011 report[7] that “a dozen activists gather outside of Raytheon Missile Systems in Tucson, Arizona at 7:00 a.m. Monday morning October 3 to protest drone warfare and war profiteering.” In a second example, an article[8] reports a protest speech at Lockheed Martin facilities that lamented, “Lockheed Martin is the world's largest war profiteer with income exceeding $42.7 billion annually.” The speech went on to say, “Today, we stand before Lockheed Martin, the world's largest war profiteer and weapons corporation, remembering all the victims of war and the economy of weapons building, remembering all the casualties of social and environmental neglect, remembering and mourning all who suffer and die on the altar of corporate greed, empire, poverty, and violence.” The article also notes that protestors walked onto company property with large signs reading “Many Suffer, Few Profit.” Another anti-war profiteering protest against Lockheed Martin saw sixteen protestors arrested on April 10, 2009.[9] In a third example, on March 19, 2007, CNN Money reported that anti-war protestors claiming to represent “hundreds of groups across the country carrying ‘blood-spattered’ cardboard signs displaying the record profits of companies (Boeing, General Dynamics and Halliburton) were arrested outside of the New York Stock Exchange” after blocking security checkpoints and area traffic.[10] In a fourth example, on March 19, 2008, USA Today reported that Students for a Democratic Society protesters marched up and down K Street in Washington, D.C., as part of their “Funk the War” protest to “put on the map all the people who profited from the war.”[11] In a fifth example, on March 19, 2007, Reuters reported protestors in San Francisco and New York City chanting, “stop the money, stop the war,” in “their protest at major defense contractors Lockheed Martin, Boeing, Northrop Grumman, Halliburton, General Electric and others.” Reuters reported a protestor lamenting that “U.S. service members and Iraqi civilians are dying so that an elite few can profit.”[12]

Recurring legislative debates targeted war profiteers amid these protests. On September 30, 2003, Senator Leahy proposed the War Profiteering Prevention Act of 2003, an amendment that prohibited and criminalized excessive profiteering related to military action, relief and reconstruction efforts with fines up to twice the profit deemed excessive. The amendment was accepted by the Senate Appropriations Committee and reported to the Senate. On October 17, 2003, the Senate passed the amendment. However, the amendment was later deleted. Versions of the amendment were reintroduced on November 3, 2003; March 2, 2006; and January 4, 2007, as U.S. casualties in Iraq hit 1000 in September 2004, 2000 in October 2005 and 3000 in December 2006. Each amendment shared common language targeting the prohibition of profiteering related to “military action, relief and reconstruction efforts in Iraq.”[13] These amendments were motivated by war profiteering that had “again plagued this nation during the engagement of U.S. forces in Iraq and Afghanistan” (War Profiteering Prevention Act of 2007, S. 119, 110th Congress). Representative Henry Waxman argued in Congress that,

> The fiasco in Iraq was a windfall for some. Halliburton made more than $2 billion in profits last year. Its total revenue has increased by 66 percent since 2002 …. Well, the American people might think that Congress would rise up against such unconscionable profiteering. When our troops are willing to sacrifice so much, and they do sacrifice so much, how can we let others create cynical fortunes off their blood? (Congressional Record, June 15, 2006, H4035)

[6] The conspicuous absence of new war taxes during the Vietnam War is an artifact of President Johnson's advocacy for social programming stemming from his vision for a “Great Society.” Massive new spending for social programming coincided with the escalation of the Vietnam War in 1964 after the Gulf of Tonkin incident. Vietnam War escalation was the “typical” time to raise new taxes (as American politicians of all stripes “rally ‘round the flag”), but President Johnson knew that political friends and enemies alike would demand curtailed social programming expenditures before any tax increases, so he proposed no new war taxes, and the war was deficit financed during its early years. President Johnson and, later, President Nixon adopted new taxes in 1968 as war costs mounted, though these new taxes targeted inflation at home, were not characterized as war taxes, and did not target war profiteers in particular (Bank et al., 2008, p. 126–143).

[7] http://www.nukeresister.org/2011/10/03/drone-protest-leads-to-arrest-at-tucsons-raytheon-missile-systems/.

[8] https://www.nukeresister.org/2011/01/21/nonviolent-resistance-at-lockheed-martin-on-martin-luther-king-day/.

[9] See “Two Thousand Six Hundred Activists Arrested in Protests,” Bill Quigley, Huffington Post, May 25, 2011.

[10] See https://money.cnn.com/2007/03/19/markets/nyse_protest/.

[11] See https://usatoday30.usatoday.com/news/nation/2008-03-19-war-protests_N.htm.

[12] See https://www.reuters.com/article/us-iraq-protests/more-than-100-arrested-in-iraq-protests-idUSN1931980520070319.

[13] See https://www.congress.gov/bill/108th-congress/senate-bill/1813/text for text used in 2003; https://www.congress.gov/congressional-report/110th-congress/house-report/353/1 for text used in 2007.

The House of Representatives approved the 2007 version of the War Profiteering Prevention Act on October 10, 2007, with a 375–3 passage vote (GovTrack.us, 2018). Despite its popularity in the House of Representatives, it would eventually die in Senate over disagreement on the definition of the threshold above which profits become "excessive."[14] There would not be a fifth attempt to curb war profits as the pace of fatalities declined markedly after 2007 (from 988 in 2007 to 454 in 2008). Nonetheless, both in and out of our sample period, U.S. history demonstrates the palpable threat of new political costs for defense firms during war.[15]

3. Related literature

Positive accounting theory predicts that taxes, regulations, management compensation plans, information costs and political costs determine accounting choices (Watts and Zimmerman, 1978). Political costs are government-imposed transfers of wealth from the private sector. The political cost hypothesis predicts that firms will exploit discretion in accounting choices to orchestrate the appearance of lower profits in response to an increase in the threat of political costs (Watts and Zimmerman, 1978, 1986).[16]

Early tests of the political cost hypothesis use firm size and profit as a proxy for political costs. The still-relevant critique of these proxies is that they are endogenous to the firm, which inhibits inferences regarding the causal effect of political costs on accounting choices (Demski, 1988, p. 625; Watts and Zimmerman, 1990, p. 131). Method advances over the past four decades improved inference in the political cost literature. For example, the introduction of cross-sectional analyses improved the credibility of inferences (see, e.g., Wong, 1988; Rayburn and Lenway, 1992; Northcut and Vines, 1998; Patten and Trompeter, 2003; Grace and Leverty, 2010; Mills et al., 2013).[17] More recent investigations further improved the credibility of inferences by using shock-based variation in the threat of political costs. Ultimately, however, these shock-based designs are susceptible to confounds because the examined shocks are not random. In their description of shock-based research designs permitting causal inference, Atanasov and Black (2016, p. 291) conclude, "All seek to approach the ideal of random assignment of treatment. All therefore require that the shock be as good as randomly assigned." Without the use of a random source of variation in the threat of political costs, causal inference remains out of reach.

We structure our discussion of the prior literature by grouping papers by how the random assignment condition is violated. Violations largely stem from the use of sources of variation in the threat of political costs 1) to which the firm elects to subject itself or 2) that can be influenced or predicted by the firm. In addition, we discuss another impediment to causal inference: that sources of variation in the threat of political costs often contemporaneously affect firm performance. Earnings management inferences are confounded because the source of variation in political costs simultaneously biases discretionary accruals in the direction predicted by the political cost hypothesis.[18] Our intent with this discussion format is not to be overly critical because the prior literature did recognize and employ tactics to address endogeneity concerns when settings permitted. Nonetheless, limitations remain. The purpose of this discussion is to identify scope for increasing the credibility of inferences in the political cost literature.

The first group of papers violates the as-if-random assignment of political costs because treatment (a change in political costs) is initiated by the firm. Examples include Jones (1991); Magnan et al. (1999); and Godsell et al. (2017). These studies examine firms that choose to petition for an import relief investigation or an antidumping complaint in the U.S., Canada and Europe, respectively. Similarly, Lim and Matolcsy (1999) and Navissi (1999) study the accrual choices of firms that request an increase in the permissible price for goods from the Australian Prices Justification Tribunal and firms that apply for price increases in New Zealand after the 1970 Price Freeze Regulation, respectively. Relatedly, Cahan (1992), Johnston and Rock (2005), and Hall and Stammerjohan (1997) study the earnings response of firms undergoing an antitrust investigation; firms potentially violating the Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA); and oil companies undergoing litigation, respectively. In these settings, firms select themselves, or litigators and regulators select firms based on firm characteristics, so these settings do not address concerns that firms' susceptibility to political costs is endogenous to their policies.

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[14] Previous Congresses overcame this obstacle. For example, Congress held that 2.5% was a reasonable profit during the War of Independence and that 3.5%–6.0% was a reasonable profit during World War I (Brandes, 1997, p. 8).

[15] Political costs also manifest as salary restrictions, which directly affect defense firm managers' wealth. Congress proposed limiting wartime salaries to $10,000 in 1935 (the equivalent of $185,401 in 2018 dollars). In 1942, President Roosevelt issued an executive order limiting salaries to $25,000 ($401,506 in 2018 dollars) (Brandes, 1997, p. 254). By contemporary standards, the restrictions would also be binding. More recently, Representative Waxman, quoted above, lamented in Congress, "… David Brooks. He is the CEO of a company that makes bulletproof vests. In 2001, Mr. Brooks reportedly earned $525,000. In 2004, he earned $70 million."

[16] We do not condition this statement on the level of pre-treatment profits or return on assets, because return on assets is not the only metric observed by the public. Watts and Zimmerman (1978, p. 115, footnote 12) cite Congressional and Senate testimony by Professor Mencke of Tufts University, who states that "absolute and not relative accounting profits are the relevant variable for explaining political action against corporations."

[17] Mills et al. (2013), for example, examine the relationship between procurement firms' sensitivity to political costs and effective tax rates (ETRs) and posit that more politically sensitive firms will report higher ETRs. The proxies for political sensitivity examined in this study are the size of procurement contracts and the ratio of procurement sales to total sales. A concern is that procurement firms have substantial influence over the test variables: contract size, procurement sales ratio and ETR.

[18] We discuss how our research design is not susceptible to these confounds in Section 6.

The second group of papers includes those in which firms influence the probability of treatment. Firms have strong incentives to, and channels by which they can, influence variation in the threat of political costs. Public policy drives political costs, and policy shocks are subject to firm influence through lobbying and other forms of political activism. For example, state-level legislation is frequently subject to firm influence (see, e.g., firm influence over the state-level adoption of anti-takeover laws [Catan and Kahan, 2016; Karpoff and Wittry, 2018; Werner and Coleman, 2015]). If corporate decisions influence whether a firm is more or less likely to endure a political cost shock, then political costs are not as-if-randomly assigned. Monem (2003) uses the introduction of the Australian Gold Tax as a source of variation in political costs. Key (1997) tests the earnings management response of 24 cable television providers during a period of industry congressional scrutiny. Hao and Nwaeze (2015) examine earnings management by pharmaceutical companies in anticipation of the 2008 presidential election, which proposed major healthcare reform. Ramanna and Roychowdhury (2010) examine outsourcing firms' discretionary accruals around an election in which outsourcing was a major campaign issue. Cahan et al., (1997) estimate earnings management in anticipation of the CERCLA of 1980 on chemical companies. In each of these studies, the threat of political costs could be influenced by politically active firms in the industry, resulting in an endogenous shock. Anticipation of variation in the threat of political costs similarly violates random assignment of political costs. Treatment firms may have anticipated the gold tax, which was preceded by exceptionally high gold prices; cable television scrutiny, which was preceded by successive rate increases; major electoral issues, which are often well known in advance of elections; and the CERCLA, which followed the Resource Conservation and Recovery Act of 1976 after public concern over toxic waste dumping. Because these papers examine political costs the firm can influence or anticipate, they do not address concerns that firms' susceptibility to political costs is endogenous to its policies.

Finally, several studies examine sources of variation that have a direct effect on firm profits. If the source of variation in political costs has a direct effect on profit, then researchers cannot disentangle accruals recorded to recognize the direct economic effect of political costs from accruals recorded to reduce the threat of future political costs. For example, Godsell et al. (2017) caveat that the income-decreasing accruals observed around EU import relief investigations are indistinguishable from genuine economic injury potentially suffered by import relief petitioners. Key (1997, p. 311) raises a similar concern regarding Jones (1991):

> Jones (1991) examines the behavior of managers applying to the International Trade Commission for import relief. There are potential problems in interpreting her results because correlated firm performance potentially biases discretionary accruals in the predicted direction of earnings management.

Similarly, Hall (1993); Han and Wang (1998); and Byard et al. (2007) examine accruals around oil price spikes. In all three studies, oil price spikes are argued to vary the threat of political costs for petroleum firms, but they also affect firm performance. Income-decreasing accruals around oil price spikes could instead be attributed to income smoothing, where firms record income-decreasing accruals when earnings are high and reverse them when earnings are low (Beidleman, 1973).

Overall, these papers demonstrate associations but cannot (and seldom claim to) generate causal inferences regarding whether the political cost hypothesis explains accounting choices. Leuz and Wysocki (2016) note that myriad studies are not a replacement for random variation:

> One might argue that we often have several empirical studies providing consistent results … and that consistent evidence should make us more confident that certain economic links exist …. However, this “piling” up of studies does not address the fundamental challenges limiting causal inferences unless the different studies have fairly orthogonal research design challenges. In our judgement, studies often share fairly similar identification and measurement problems, and, hence, different studies do not really “diversify” the research-design problem. (p. 533)

The foregoing describes the current state of the political cost literature. Our design extends the literature by addressing the concern that firms' susceptibility to political costs is endogenous to its policies. We address this research design challenge by using a source of variation in political costs that randomly affects firms across time and place. Random variation in the threat of political costs addresses endogeneity concerns because such variation can only be randomly associated with firm policies.

4. Hypothesis development

Empirical studies investigating the association between soldier fatalities and public awareness of the substantial human costs of war are tests of the mortality salience hypothesis (Mueller, 1973; Burk, 1999). The mortality salience hypothesis suggests that dead soldiers are the most salient war cost and, as such, attract public attention to the substantial human costs of war. Mueller (1973), Gartner (2008), and Gartner and Segura (1998, 2000; 2008) examine the public response to U.S. wars and, consistent with the mortality salience hypothesis, find that voter support for the war declined as casualties mounted. Eichenberg et al., (2006); Karol and Miguel (2007); and Mueller (2005) further find that growing national casualties reduce public support for incumbent politicians. Others show that fatalities affect foreign policy decisions (Carson et al., 2001; Gartner et al., 2004; Myers and Hayes, 2010).

In related work, Gartner and Segura (2000) and Gartner et al. (1997) form the *proximate* casualties hypothesis. The proximate casualties hypothesis argues that the effect of local soldier fatalities on political outcomes is larger relative to non-local soldier fatalities because personal ties to the deceased are stronger and more numerous in the deceased soldier's home state. In empirical studies, Althaus et al. (2012), Kriner and Shen (2010), and Hayes and Myers (2009) find support for the proximate casualties hypothesis. They find that local soldier fatalities elicit more local public awareness of the war than national fatalities. Gartner (2008) finds that personal relations of deceased soldiers are more likely than others to question U.S. policy and disapprove of the U.S. president. Other studies find that local voter turnout increases and local voter approval ratings decline for incumbent politicians after increases in local soldier fatalities (Koch and Nicholson, 2016; Kriner and Shen, 2010; Valentino et al., 2010; Hayes and Myers, 2009; Karol and Miguel, 2007). Consistent with fatalities increasing awareness of the war effort, the authors find that voters are more likely to cast a ballot subsequent to more local and more recent fatalities.

Public awareness of the substantial human costs of war increases public opposition to war profits because war profits amid dead soldiers violate the U.S. tradition of shared wartime sacrifice. Public opposition to war profit is a source of political costs for defense firms because political costs are determined by public policy, and public policy is determined by politicians accountable to democratic institutions such as “public opinion, public debate, rallies and protests” (Avant and Sigelman, 2010, p. 236). If local defense firms anticipate that war profiteering against the backdrop of large increases in local soldier fatalities increases the threat of political costs, then local defense firms have a strong incentive to dull public outrage and reduce the threat of political costs by orchestrating the appearance of lower profits. Accordingly, and consistent with the political cost hypothesis, we expect local defense firms will record income-decreasing accruals when local soldier fatalities are higher.

5. Data

5.1. Procurement data

We draw procurement data from approximately 48 million government contracts available through the website USAspending.com. The website acts as a point of entry for the Federal Procurement Data System—Next Generation (FPDS-NG), which aggregates federal government procurement contracts. The FPDS has existed since 1979, and the Next Generation postfix reflects the introduction of a user-friendly interface in 2003. With the introduction of the FPDS-NG, the cost of acquiring information about defense contracts had never been lower. The FPDS-NG is a comprehensive database listing contracts with values greater than $3000 ($25,000 for contracts dated before 2004). The FPDS-NG provides detailed procurement contract data including the value of the contract, the number of bids, the date on which the contract was awarded, the type of product or service procured, and the agency that awarded the contact. We code firms as defense firms if more than 5 percent of firm sales were drawn from defense contracts in the pre-war period (year 2000).[19]

On one hand, defining DOD contractors in 2000 has the advantage that it excludes firms that endogenously choose to contract with the DOD in response to the wars in Iraq and Afghanistan. For example, firms less susceptible to political costs may choose more often to enter the defense industry after war commences. This selection effect threatens our identification strategy. On the other hand, classifying DOD contractors in 2000 may misclassify firms as defense firms if they cease supplying war materiel to the DOD during the sample period. This measurement error also threatens our identification strategy. Essentially, we are choosing between a selection problem and a measurement error problem in constructing our definition of defense firms. We manually check and find that only 1 of the 77 defense firms identified in year 2000 does not supply war materiel to the DOD during the sample period. Consequently, we prioritize mitigation of the selection problem by identifying DOD contractors in the pre-war period.

Linking public firms named in the FPDS-NG data to public firms named in Compustat North America involves several steps. First, we identify the parent company recorded in the FPDS-NG data. Second, we use a matching algorithm to match the name of the contractor with a list of publicly traded company names from Compustat. Our matching algorithm strips common strings (e.g., “Inc.,” “Ltd.”), punctuation and spaces from firm names in both databases before comparing the two data strings. Third, the algorithm compares each company name in the FPDS-NG to each company name in Compustat. The algorithm analyzes each consecutive letter of every name pair − one from the FPDS-NG, the other from Compustat − and a similarity score ranging from zero to one is computed where the score is zero if no letters match and one if all letters match. Fourth, the algorithm generates a list of matches from highest to lowest score. We retain all matches with a score in excess of 0.30 and hand-check each match to remove erroneous matches.[20] Finally, we use the Data Universal Numbering System (DUNS) firm identifier to identify companies that have changed names or have names incorrectly keyed in the FPDS-NG database. Specifically, we aggregate the DUNS numbers for each public firm and identify contracts awarded to these DUNS identifiers. This process identifies public firm contracts missed by the fuzzy-matching algorithm. We identify 398 (2823) distinct defense firms (firm-years) with defense procurement contracts using FPDS-NG procurement data. Of these 398 distinct defense firms, 77 have defense revenues that exceed 5 percent of total sales.[21]

[19] Contrary to popular belief, sales to the DOD, on average, comprise a small fraction of total sales for most defense firms. Because wars are infrequent and unpredictable in duration, few firms that shape their entire product market strategy around the manufacture and sale of war matérial survive in the long term (Brandes, 1997).

[20] For example, “Advanced Energy Industries, Inc.” in the FEC database is stripped of common words, abbreviations, spaces and punctuation to be read by the computer as “Advanced Energy.” This truncated string is matched to the Compustat name “Advanced Energy Inds Inc.,” which is read by the computer as “Advanced Energy Inds.” The match score is imperfect at 0.764 due to the absence of character match on the final four characters in the Compustat firm name. However, because the score is above 0.30, we manually check and confirm the match for inclusion in our sample.

[21] We later discuss the robustness of our results to variation in this threshold.

5.2. Soldier fatality data

We provide nationwide soldier fatality counts by year and state in Panel A of Table 1. We draw data on 6418 soldier fatalities from www.icasualties.org. This data source lists every fatality in Operation Iraqi Freedom and Operation Enduring Freedom. The panel includes the deceased's service branch, component (active/reserve/guard), name, rank, pay grade, date of death, hostile status of death, age, gender, home of record (HOR) city, HOR county, HOR state, HOR country, unit, incident geographic code, casualty geographic code, casualty county, city of loss and race/ethnicity. We summarize soldier fatality data in Panel A of Table 1. Major war engagement in 2003 led to 510 soldier fatalities, increasing to 877 and then 918 in 2004 and 2005, respectively, before peaking in 2007 at 988 fatalities and then moving to approximately 500 per year until 2011 before dropping further.

We also report state-level fatality data and firm headquarter data by permanent residence state in Panel A of Table 1. Soldier fatalities are local to defense firms in our sample when the deceased soldier's permanent residence is in the same state as the war profiteer. To address concerns that Compustat only reports the current headquarter state, we rely on historical SEC filings to identify the historical firm headquarter for each year in our sample (Heider and Ljungqvist, 2015).[22] Soldier fatalities are widely distributed across states. Defense firm headquarters appear to be widely distributed across 21 states, although concentrations exist in California and New York. We later discuss the robustness of our results to the removal of firms in these states.

5.3. Firm characteristics

We draw firm-level financial statement data from Compustat North America. Panel B of Table 1 reports the sample construction to facilitate replication. We draw 40,160 firm-years for firms spanning the 2003–2012 period from Compustat North America. After removing firms with non-U.S. headquarters, regulated firms, firm-years in 2-digit SIC and year clusters with fewer than 20 firms, and firms with missing test and control variables, our estimation sample numbers 2932 distinct firms and 16,749 firm-years. Of this panel, 77 firms (582 firm-years) are defense firms.

Panel A of Table 2 provides an industry (Fama French 10 classification, for brevity) breakdown of our sample. Consistent with Mills et al. (2013) and Samuels (2020), who examine procurement firms, the defense firms in our sample are concentrated among manufacturing and high-tech firms. Panel B of Table 2 provides a year breakdown of our sample.

We include an exhaustive list of accrual determinants in our empirical models so that we can better isolate discretionary accruals recorded by local defense firms around local soldier fatalities. We winsorize all continuous variables at 1 and 99 percent. Table 3 presents summary statistics for publicly traded firms over the sample period of 2003–2012. Our dependent variable is discretionary accruals. The mean of discretionary accruals approximates zero for both defense and non-defense firms. Our source of variation in political costs, which varies at the state-year level, is local soldier fatalities. Local soldier fatalities divided by the state population (millions) averages a statistically indistinguishable 2.035 (2.041) for non-defense (defense) firms. The standard deviation of our measure of local soldier fatalities is 1.06. We measure the economic significance of our results by describing the change in accruals after a one standard deviation increase in local soldier fatalities per capita. In nominal terms, a one standard deviation increase in local soldier fatalities for the average state year reflects an additional 28.51 local soldier fatalities.

Our prior is that firms in defense and non-defense industries will have distinct firm characteristics. The tabulation of summary statistics reported in Table 3 confirms this prior. Consequently, we include the long list of known accrual determinants listed in Table 3 in our estimations and test the robustness of our results in a performance-matched sample, an entropy-balanced sample, and a defense firm-only sample in Online Appendices 1 to 3, respectively.

Table 4 reports correlation coefficients between variables examined in our main analysis. The full sample correlation between our measure of local soldier fatalities and discretionary accruals is near zero at 0.0038 and statistically insignificant. Our two-stage measure of discretionary accruals is orthogonal to first-stage accrual determinants (1/Total Assets; ΔRevenue − ΔAccounts Receivable; Property, Plant and Equipment, Gross; ROA). Correlations are generally low for other variable pairs.[23]

6. Research design

The political cost hypothesis predicts that firms will exploit discretion in accounting choices to orchestrate the appearance of lower profits when threatened with political costs (e.g., excess-profit taxes, price controls). To test this hypothesis, we ask,

[22] Consistent with Heider and Ljungqvist (2015), 10 percent of our sample has Compustat header state headquarter locations that do not match the historical headquarter state drawn from SEC filings.

[23] The correlation table displays a statistically significant correlation between local soldier fatalities and some firm characteristics. One or more time-invariant state-level and/or time-varying industry-level factors associated with both local soldier fatalities and firm performance cause these correlations. We find no correlation between firm characteristics and a measure of abnormal local soldier fatalities defined as the local soldier fatalities de-meaned by state-level and industry × year level means of local soldier fatalities. To address concerns about potential correlated omitted variables, we use state fixed effects (or firm fixed effects, within which headquarter state does not vary for 90% of firms) as well as industry × year fixed effects throughout our analysis.

Table 1
Defense Parameters and Estimation Sample Construction.

Panel A: Soldier Fatalities by Year and State

Year	Soldier Fatalities
2003	510
2004	877
2005	918
2006	901
2007	988
2008	454
2009	458
2010	548
2011	460
2012	304
Total	6418

State	Soldier Fatalities	Defense Firm-Years	State	Soldier Fatalities	Defense Firm-Years
AK	22	0	MT	37	0
AL	101	0	NC	184	10
AR	89	0	ND	19	0
AZ	144	0	NE	60	0
CA	699	145	NH	37	0
CO	94	0	NJ	126	27
CT	49	10	NM	54	0
DC	7	0	NV	52	0
DE	16	0	NY	279	72
FL	333	10	OH	266	4
GA	204	19	OK	124	0
HI	38	0	OR	100	10
IA	71	9	PA	281	16
ID	46	0	RI	18	0
IL	243	13	SC	34	0
IN	147	0	SD	26	0
KS	71	0	TN	137	0
KY	108	0	TX	587	29
LA	120	0	UT	43	4
MA	118	44	VA	196	48
MD	118	25	VT	25	0
ME	44	0	WA	143	33
MI	224	11	WI	121	30
MN	95	13	WV	35	0
MO	139	0	WY	20	0
MS	74	0			

Panel B: Estimation Sample Construction

CRSP-Compustat firm-years 2003–2012		**40,160**
Less: Firms with headquarters outside the United States	(6882)	33,278
Less: Regulated firms	(4263)	29,015
Less: Firms in 2-digit SIC–year clusters with fewer than 20 firms	(2661)	26,354
Less: Firms with missing test and control variables	(9605)	16,749
Firm-years in panel used to test H_1:	**16,749**	
	Full Sample	
Defense (Treatment) Firm-Years:	582	
Non-Defense (Control) Firm-Years:	16,167	
Total Firms:	16,749	

Panel A reports the frequency of fatalities by year and state. Panel B describes our sample selection process showing sample formation from data retrieval to estimation.

do firms record income-decreasing accruals when there are increases in local soldier fatalities? To answer this research question, we estimate Equation (1).

$$\begin{aligned} \textit{Discretionary Accruals}_{i,t} = {} & B_0 + B_1 \textit{Local Soldier Fatalities}_{s,t} \times \textit{Defense Firm}_i + B_2 \textit{Local Soldier Fatalities}_{s,t} \\ & + B_3 \textit{Defense Firm}_i + a_{j\times t} + a_i + \gamma X_{it} + \varepsilon_{it} \end{aligned} \quad (1)$$

Table 2
Estimation sample by industry and year.

Panel A: Fama-French 10 Industry Breakdown

	Control Firms		Defense Firms with > 5% Procurement Sales Ratio	
Fama-French industry code (10 industries)	Frequency	%	Frequency	%
Non-Durable	960	6%	10	2%
Consumer Durable	614	4%	0	0%
Manufacturing	2808	17%	115	20%
Energy	882	5%	10	2%
High Tech	4715	29%	367	63%
Wholesale Retail	1806	11%	15	3%
Health	3054	19%	8	1%
Other	1328	8%	57	10%
Total	16,167		582	

Panel B: Year Breakdown

Year	Control Firms		Defense Firms with > 5% Procurement Sales Ratio	
	Freq.	%	Freq.	%
2003	2067	13%	62	11%
2004	1900	12%	65	11%
2005	1754	11%	69	12%
2006	1632	10%	63	11%
2007	1571	10%	63	11%
2008	1520	9%	61	10%
2009	1507	9%	54	9%
2010	1487	9%	51	9%
2011	1401	9%	48	8%
2012	1328	8%	46	8%
Total	16,167		582	

This table reports defense and non-defense firm-years by industry and year.

Table 3
Estimation sample summary statistics.

Variable	Control Firms				Defense Firms with > 5% Procurement Sales Ratio (Treatment)				Test of Difference in Means
	Mean	Median	St. Dev.	N	Mean	Median	St. Dev.	N	*t*-stat
Discretionary Accruals	0.000	−0.001	0.076	16,167	0.008	0.003	0.075	582	(2.59)
Soldier Fatalities	2.035	1.866	1.062	16,167	2.041	1.844	1.036	582	(0.14)
1/Total Assets	0.013	0.003	0.027	16,167	0.010	0.003	0.021	582	2.93
ΔRevenue - ΔAccounts Receivable	0.054	0.056	0.214	16,167	0.057	0.062	0.179	582	(0.27)
Property, Plant and Equipment, Gross	0.487	0.368	0.388	16,167	0.328	0.287	0.247	582	9.81
ROA	0.016	0.069	0.223	16,167	0.006	0.073	0.230	582	1.08
Cash Flows_t	0.045	0.084	0.192	16,167	0.019	0.072	0.193	582	3.30
Size	5.783	5.703	1.931	16,167	6.267	5.858	2.171	582	(5.92)
Book Leverage	0.172	0.124	0.190	16,167	0.127	0.098	0.128	582	5.62
Sales Growth	0.072	0.042	0.207	16,167	0.070	0.051	0.149	582	0.17
Employee Growth	0.062	0.029	0.237	16,167	0.070	0.048	0.214	582	(0.81)
Std (Sales)	0.139	0.094	0.141	16,167	0.125	0.086	0.120	582	2.47
NOA_{t-1}	0.304	0.075	0.779	16,167	0.142	0.056	0.254	582	5.00
Market Return	0.244	0.068	0.866	16,167	0.156	0.063	0.656	582	2.42
Market-to-Book Ratio	2.878	2.045	3.959	16,167	2.861	2.126	3.281	582	0.10
Loss	0.324	0.000	0.468	16,167	0.287	0.000	0.453	582	1.90
Operating Cycle	3.731	2.830	24.438	16,167	6.126	5.887	22.844	582	(2.33)
Stock Volatility	0.740	0.429	1.085	16,167	0.577	0.361	0.736	582	3.60
Standard Deviation of Earnings	0.087	0.037	0.135	16,167	0.062	0.023	0.099	582	4.38
Standard Deviation of Cash Flows	0.070	0.043	0.085	16,167	0.055	0.037	0.058	582	4.37
Industry Adjusted Earnings	−0.015	0.009	0.169	16,167	−0.047	−0.003	0.172	582	4.43
Idiosyncratic Shocks	0.021	0.011	0.032	16,167	0.016	0.009	0.026	582	4.06

This table reports summary statistics for the sample. All variables defined in Appendix A.

Table 4
Correlation table.

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
Discretionary Accruals	1	1																				
Local Soldier Fatalities	2	0.0038	1																			
1/Total Assets	3	0.0000	−0.0293*	1																		
ΔRevenue - ΔAccounts Receivable	4	0.0000	0.0986*	−0.0669*	1																	
Property, Plant and Equipment, Gross	5	0.0000	0.0497*	−0.0036	−0.0560*	1																
ROA	6	0.0000	0.0479*	−0.4655*	0.2247*	0.0451*	1															
Cash Flows$_t$	7	−0.2123*	0.0362*	−0.4283*	0.1427*	0.1594*	0.8865*	1														
Size	8	−0.0157	−0.0148	−0.6386*	0.0000	0.1121*	0.3743*	0.3477*	1													
Book Leverage	9	0.0449*	0.0101	−0.1478*	−0.0286*	0.2338*	0.0017	−0.0283*	0.2850*	1												
Sales Growth	10	0.0399*	0.0761*	0.0044	0.5454*	−0.0876*	−0.0139	−0.0535*	−0.0786*	0.015	1											
Employee Growth	11	0.0452*	0.0735*	−0.0652*	0.3313*	−0.1119*	0.0921*	0.0618*	−0.0345*	0.0032	0.3693*	1										
Std (Sales)	12	0.0052	−0.0237*	0.2053*	0.0336*	−0.0726*	−0.0887*	−0.0907*	−0.2390*	−0.0263*	0.0364*	0.0418*	1									
NOA$_{t-1}$	13	0.0227*	0.0216*	−0.017	−0.0114	0.0945*	−0.2655*	−0.2530*	0.0517*	0.4639*	0.2226*	0.0199	−0.1203*	1								
Market Return	14	0.0127	−0.0749*	0.0932*	0.0548*	−0.005	−0.0172	−0.0132	−0.1368*	−0.019	0.0724*	0.0624*	0.0793*	0.0215*	1							
Market-to-Book Ratio	15	−0.0223*	0.0197	0.0731*	0.0905*	−0.0961*	−0.0519*	−0.0609*	−0.0484*	−0.0684*	0.1210*	0.0903*	0.0166	−0.002	0.1359*	1						
Loss	16	−0.0709*	−0.0610*	0.2654*	−0.2559*	−0.0348*	−0.6164*	−0.5322*	−0.3073*	0.0509*	−0.0341*	−0.1067*	0.1280*	0.1851*	−0.0081	0.0047	1					
Operating Cycle	17	0.3107*	0.0774*	−0.0885*	0.2561*	−0.0718*	0.1466*	0.0312*	0.0048	0.0157	0.3427*	0.3952*	−0.0499*	0.0363*	0.0301*	0.0520*	−0.1355*	1				
Stock Volatility	18	0.0006	0.0124	0.1298*	0.0344*	−0.0551*	−0.1340*	−0.1248*	−0.2019*	−0.0279*	0.0953*	0.0889*	0.1282*	0.0256*	0.0042	0.0418*	0.1194*	0.0320*	1			
Standard Deviation of Earnings	19	0.0149	−0.0674*	0.3646*	−0.1119*	−0.0606*	−0.5527*	−0.4773*	−0.3444*	−0.0303*	0.0829*	−0.0538*	0.2845*	0.1461*	0.1560*	0.0968*	0.4013*	−0.0839*	0.1770*	1		
Standard Deviation of Cash Flows	20	0.0316*	−0.0406*	0.3799*	−0.0117	−0.1306*	−0.4763*	−0.4701*	−0.4219*	−0.1056*	0.1529*	0.0365*	0.2612*	0.1089*	0.1273*	0.1223*	0.3065*	−0.011	0.2253*	0.5415*	1	
Industry Adjusted Earnings	21	−0.2313*	0.0426*	−0.4090*	0.1586*	0.0997*	0.8414*	0.9486*	0.3408*	−0.0292*	−0.0223*	0.0606*	−0.1036*	−0.2167*	−0.0156	−0.0289*	−0.5252*	0.0372*	−0.1217*	−0.4429*	−0.4234*	1
Idiosyncratic Shocks	22	0.0052	−0.0911*	0.3240*	−0.0959*	−0.0029	−0.3426*	−0.3015*	−0.3494*	0.0249*	0.0325*	−0.0666*	0.1761*	0.0953*	0.3975*	0.0418*	0.3117*	−0.0802*	0.1242*	0.3771*	0.2983*	−0.2985*

This table reports variable correlations. * denotes two-tailed significance levels at the 1% level. All variables defined in Appendix A.

where *Discretionary Accruals* is a measure of firms' abnormal accruals equal to the residual of an abnormal accruals model. The residual of the performance-adjusted (Kothari et al., 2005) modified-Jones (1991) model (Dechow et al., 1995) is our proxy for discretionary accruals.

$$Total\ Accruals_{it} = a_0 + a_1\frac{1}{AT_{it-1}} + a_2(\Delta REV_{it} - \Delta AR_{it}) + a_3 PPE_{it} + a_4 ROA_{it} + \varepsilon_{it} \tag{2}$$

where *Total Accruals* for firm *i* in year *t* are calculated as the change in current assets plus change in debt included in current liabilities, less change in cash, change in current liabilities, and depreciation and amortization expense (Kothari et al., 2016; Ecker et al., 2013). ΔREV_{it} is the change in sales (SALE) from year $t-1$ to year t; ΔAR_{it} is the change in accounts receivable (RECT) from year $t-1$ to year t; PPE_{it} is gross property, plant and equipment (PPEGT); and ROA_{it} is return on assets calculated as the income before extraordinary items (IB) divided by lagged total assets (AT_{t-1}). We scale variables by lagged total assets. We estimate Equation (2) for each industry-year with more than 20 firm-year observations. ε_{it} is the error term and our measure of discretionary accruals.

Importantly, the dependent variable in Equation (1), *Discretionary Accruals*, is orthogonal to the economic effects of time-varying industry-level political costs because political costs imposed on the defense industry vary at the industry-year level. Consequently, a_0 in Equation (2) captures the direct economic effect of temporal variation in the threat of industry-level political costs on accruals. As a result, ε_{it} from Equation (2), our measure of discretionary accruals, is orthogonal to the direct economic effects of variation in the threat of industry-level political costs on accruals.[24] Orthogonality between discretionary accruals and the direct economic effects of variation in the threat of political costs allows us to isolate local defense firms' discretionary accrual-based response to increases in the threat of political costs, thereby permitting well-identified tests of the political cost hypothesis.

Defense Firm is an indicator variable equal to one when the firm has sales to the DOD in excess of 5 percent of all sales in the pre-war year of 2000 and zero otherwise. Our state-year level proxy for political costs, *Local Soldier Fatalities*, is the quotient in Equation (3):

$$Local\ Soldier\ Fatalities_{s,t} = Number\ of\ Soldier\ Fatalities_{s,t} \div State\ Population\ (millions)_{s,t} \tag{3}$$

where *Number of Soldier Fatalities* is the number of local soldier fatalities in the twelve months leading up to firm *i*'s fiscal year-end in year *t*. Fatality data from icasualties.org include the HOR (the permanent residence) of the deceased soldier.[25] *State Population* is the population of state *s* in year *t*. Thus, *Local Soldier Fatalities* is a per-capita measure of state-year fatalities. Local soldier fatalities satisfy several identification requirements. First, local soldier fatalities lead to strong revisions in public opinion about the substantial human costs of war. Local soldier fatalities are tied to political outcomes including higher voter turnout and opposition to war (Althaus et al., 2012). Second, local soldier fatalities are exogenous to the firm. Local defense firms cannot influence the permanent residence of a deceased soldier or the timing of a soldier fatality. Third, the state of permanent residence and the year of death for the deceased soldier are not individually, much less jointly, predictable. For example, we demonstrate in Appendix Table 1A that deviations from the state-mean of soldier fatalities do not predict future deviations from the state-mean. Consequently, firms cannot change their headquarter location in anticipation of political costs spurred by future local soldier fatalities, nor can soldiers change their HOR in such a way that it correlates with state-year soldier fatalities. Fourth, we know of no local mechanism relating local soldier fatalities to local defense firms' discretionary accruals. For example, we demonstrate in Appendix Table 1B that there is no association between local soldier fatalities and local defense firms' contemporaneous procurement revenues.

Equation (1) tests whether firms manage earnings downward to reduce political costs in response to changes in the threat of political costs. Our test variable coefficient, B_1, estimates the sensitivity of defense firms' discretionary accruals to local soldier fatalities, B_2 estimates the sensitivity of discretionary accruals to local soldier fatalities for non-defense firms, and B_3 estimates the average discretionary accruals for defense firms; $\alpha_{j\times t}$ is an industry × year fixed effect that controls for time-varying industry-level phenomena concurrently affecting accruals in all firms in an industry-year. α_i is a firm fixed effect that controls for time-invariant firm-level characteristics that may affect accruals. ε_{it} is an error term.[26]

X_{it} is a suite of firm-level control variables known to be related to firms' accrual choices. We also include *Lagged Local Soldier Fatalities* × *Defense Firm* to test whether accruals unravel in a predictable way when the threat of political costs

[24] We further orthogonalize discretionary accruals with respect to the economic effects of time-varying industry-level political costs by estimating all specifications with industry × year fixed effects. A third method by which we control for the direct economic effect of variation in the threat of political costs on accruals is by restricting the sample to only defense firms and then estimating Equation (1) using year fixed effects. Political costs (e.g., excess-profit taxes, price controls) vary for all defense firms by year; consequently, year fixed effects control for the direct economic effects of variation in political costs on accruals. We replicate all tests using this sample and specification and tabulate these results in Online Appendix 3.

[25] HOR differs from the state of legal residency (SLR). HOR is where the soldier lived on the day the soldier joined the service and does not change over the course of military service. This is an important distinction as the declared SLR suffers from selection effects; the soldier may opt to declare legal residency in states with low or no state-level income tax.

[26] In tabulations of our main result in Table 5, we also show specifications without controls or fixed effects, with controls but no fixed effects, with controls and fixed effects including state and industry × year fixed effects, and, finally, our main specification described above that includes firm and industry × year fixed effects. We employ this latter specification in the large majority of our tests.

dissipates (Althaus et al., 2012). To account for time-varying firm characteristics known to be related to firms' accrual choices, we include control variables for lead, lagged and contemporaneous cash flows (Dechow and Dichev, 2002); firm size and sales growth (Jones, 1991); leverage (DeFond and Jiambalvo, 1994; Barton and Waymire, 2004); sales volatility (Francis et al., 2004); lagged net operating assets (Hirshleifer et al., 2004); market return (Zhang and Zhuang, 2012); and market-to-book ratio (Collins et al., 2017). Furthermore, recent evidence suggests that discretionary accruals need not reverse in fiscal years adjacent to their origination; Larson et al. (2018) show that lead and lagged cash flows, beyond a one-year lag or lead, help explain normal accruals. Consequently, we add two-year lead and lagged cash flow variables to Equation (2).

Another potential source of model misspecification is economic volatility. Economic volatility generates noise in the accrual-generating process and leads to spurious inferences because volatile but genuine economic fundamentals may manifest as discretionary accruals in the Jones (1991) model. For example, Owens et al. (2017) find that discretionary accrual models that do not control for idiosyncratic risk are mis-specified. We address this concern by including additional control variables capturing variation in economic activity. These variables include measures capturing firm losses, operating cycle, stock volatility, standard deviation of earnings, standard deviation of cash flows (Ball and Shivakumar, 2006; Francis et al., 2004) and idiosyncratic risk. These variables capture the direct effects of financial performance on discretionary accruals. We define all variables in Appendix A.

Overall, we expect that local defense firms will engage in income-decreasing earnings management in years with more local soldier fatalities (relative to the state-mean) and, thus, greater threat of political costs ($B_1 < 0$). The coefficient, B_1, will be statistically significant only if discretionary accruals in state-years with more local soldier fatalities differ significantly from the changes in discretionary accruals for the control firms. While deviations in fatalities from the state-mean do not predict future deviations from the state-mean (see Appendix Table 1A), the extent to which a state-year has consecutive years of soldier fatalities above the state-mean works against our hypothesis as firms cannot indefinitely manage accruals in one direction.

In our initial estimation of Equation (1), we examine a variety of specifications culminating in a specification including both firm and industry × year fixed effects. Stable test variable coefficients across model specifications corroborate as-if-random variation claims (Atanasov and Black, 2016). For example, in an investigation of the experimental Reg SHO setting by Fang et al. (2016) in which the SEC randomized treatment of firms, the test variable coefficient reported in their Table 3 does not change as control variables and fixed effect structures are added to the specification. To corroborate as-if-random variation in local soldier fatalities, we estimate Equation (1) 1) at the univariate level, 2) after including control variables, 3) after including control variables and state and industry (2-digit SIC) × year fixed effects, and 4) after including control variables and including firm and industry × year fixed effects. We expect to find a stable coefficient on our test variable, *Soldier Fatalities* × *Defense Firm*, across the four alternate specifications if local soldier fatalities are as-if-random. We cluster standard errors by firm in all tests.

7. Results

7.1. Local soldier fatalities

We estimate Equation (1) to test if local defense firms record more income-decreasing discretionary accruals when local soldier fatalities increase. We present our first results in Table 5. The test variable *Local Soldier Fatalities* × *Defense Firm* estimates the sensitivity of local defense firms' discretionary accruals to local soldier fatalities. Table 5 shows that the coefficient on the variable *Local Soldier Fatalities* × *Defense Firm* is consistently negative and statistically and economically significant. The test variable coefficient is equal to −0.0112 in Column 1, which does not include control variables or fixed effects. When we add our large suite of control variables, as shown in Column 2, the test variable coefficient is unaffected (coef.: −0.0110). Similarly, the test variable coefficient is unaffected when we add in Column 3 state and industry × year fixed effects that control for time-invariant state factors and time-varying industry factors (coef.: −0.0113). Finally, the test variable coefficient is unaffected when we add in Column 4 firm and industry × year fixed effects, which control for time-invariant firm factors and time-varying industry factors. Lack of variation in the test variable coefficient across specifications suggests that the effect of local soldier fatalities on defense firm accounting choices is orthogonal to firm characteristics (Atanasov and Black, 2016).

Column 4 reports a coefficient on our test variable, *Local Soldier Fatalities* × *Defense Firm*, of −0.0110, suggesting that a one standard deviation increase in local soldier fatalities per capita (1.06) causes an increase in income-decreasing discretionary accruals equal to 1.17 percent of total assets.[27] Given that local soldier fatalities are an as-if-random source of variation in

[27] Much concern has been expressed regarding the magnitude of earnings management inferred from discretionary accrual models throughout the earnings management literature (Ball, 2013; Zimmerman, 2013). The magnitude of earnings management we observe is plausible because it falls below materiality thresholds used by audit firms (Eilifsen and Messier, 2015). Furthermore, auditors primarily use materiality thresholds for income-increasing accruals due to the increased reputation and liability concerns associated with income-increasing accruals vis-à-vis income-decreasing accruals (DeFond and Subramanyam, 1998). If auditors apply the materiality threshold asymmetrically, then defense firms local to increases in soldier fatalities have abundant degrees of freedom in using accruals to orchestrate the appearance of lower profits.

Table 5
Defense firms' earnings management around local soldier fatalities.

	Pred.	(1)	(2)	(3)	(4)
		Discretionary Accruals			
Local Soldier Fatalities × Defense Firm	−	**−0.0112****	**−0.0110*****	**−0.0113*****	**−0.0110*****
		(-2.02)	**(-2.51)**	**(-2.55)**	**(-2.36)**
Defense Firm		0.0172	−0.0201	−0.0373	
		(0.29)	(-0.39)	(-0.73)	
Local Soldier Fatalities		0.00107	−0.0000450	0.0000419	0.000338
		(1.37)	(-0.07)	(0.06)	(0.46)
Lagged Local Soldier Fatalities		−0.000557	0.000444	−0.00147*	−0.00132*
		(-1.07)	(1.00)	(-1.93)	(-1.67)
Lagged Local Soldier Fatalities × Defense Firm	+	**0.00420**	**0.00438****	**0.00443***	**0.00486****
		(1.20)	**(1.65)**	**(1.64)**	**(1.74)**
National Soldier Fatalities		−0.000961	−0.00387**		
		(-0.45)	(-2.15)		
National Soldier Fatalities × Defense Firm		0.000922	0.00576	0.00832	0.00738
		(0.09)	(0.65)	(0.95)	(0.80)
1/Total Assets			−0.0917**	−0.0964**	−0.111
			(-2.33)	(-2.43)	(-0.89)
ΔRevenue - ΔAccounts Receivable			−0.0305***	−0.0243***	−0.0253***
			(-6.27)	(-4.61)	(-4.48)
Property, Plant and Equipment, Gross			0.0210***	0.0255***	0.0513***
			(11.45)	(10.98)	(7.26)
Return on Assets			0.229***	0.248***	0.251***
			(12.14)	(12.53)	(10.72)
Cash Flows$_{t-2}$			0.0155**	0.0140*	0.0221***
			(2.19)	(1.93)	(2.58)
Cash Flows$_{t-1}$			0.0259**	0.0222**	0.0323***
			(2.48)	(2.08)	(2.88)
Cash Flows$_{t}$			−0.355***	−0.398***	−0.306***
			(-15.90)	(-7.43)	(-3.91)
Cash Flows$_{t+1}$			0.0441***	0.0422***	0.0379***
			(4.78)	(4.50)	(3.72)
Cash Flows$_{t+2}$			0.00133	−0.00198	−0.00309
			(0.20)	(-0.30)	(-0.35)
Size			−0.00115***	−0.00108**	0.00412*
			(-2.77)	(-2.49)	(1.71)
Book Leverage			−0.00591	−0.00155	0.00973
			(-1.25)	(-0.32)	(1.15)
Sales Growth			−0.000513	−0.00191	0.00346
			(-0.09)	(-0.32)	(0.53)
Change in Employees			−0.0109**	−0.0116***	−0.00862*
			(-2.47)	(-2.59)	(-1.82)
Std (Sales)			0.000195	0.00153	0.00487
			(0.03)	(0.25)	(0.64)
NOA$_{t-1}$			0.000374	−0.000456	−0.00242
			(0.27)	(-0.31)	(-1.02)
Market Return			0.000953	0.000595	0.00120
			(1.05)	(0.60)	(1.07)
Market-to-Book Ratio			−0.000323	−0.000332	0.000172
			(-1.56)	(-1.57)	(0.62)
Loss			−0.0236***	−0.0260***	−0.0227***
			(-12.68)	(-14.00)	(-10.46)
Operating Cycle			0.000786***	0.000806***	0.000819***
			(15.79)	(15.84)	(14.30)
Stock Volatility			−0.000512	−0.000286	0.000485
			(-0.82)	(-0.46)	(0.60)
Standard Deviation of Earnings			0.0483***	0.0443***	0.0365***
			(5.98)	(5.28)	(3.28)
Standard Deviation of Cash Flows			0.00303	0.00462	0.0132
			(0.25)	(0.37)	(0.80)
Industry Adjusted Cash Flows (A)			−0.0553***	−0.0356	−0.131*
			(-4.24)	(-0.79)	(-1.87)
Negative Industry Adjusted Cash Flows (B)			0.0169***	0.0173***	0.0138***
			(10.93)	(10.83)	(7.24)
(A) × (B)			0.0264*	0.0324**	0.0265
			(1.91)	(2.05)	(1.21)
Idiosyncratic Shocks			0.0327	0.00312	0.0214
			(1.14)	(0.10)	(0.59)

(*continued on next page*)

Table 5 (*continued*)

	Pred.	(1)	(2)	(3)	(4)
		Discretionary Accruals			
Constant		0.00476	0.0272**	0.00656	−0.0488***
		(0.38)	(2.36)	(1.26)	(-2.71)
R-Squared		0.00107	0.321	0.339	0.488
Number of Observations		16,749	16,749	16,749	16,749
Standard Errors Clustered By:		Firm	Firm	Firm	Firm
State Fixed Effects		No	No	Yes	No
Firm Fixed Effects:		No	No	No	Yes
Industry × Year Fixed Effects:		No	No	Yes	Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

political costs for local defense firms, these results provide strong and robust support for the political cost hypothesis.[28] Though we cannot definitively rule out the possibility of a correlated omitted variable to explain this result, such a variable would have to vary at the state-year level with the timing of increases in local soldier fatalities from the state-mean. The as-if-random timing of soldier fatalities and the as-if-random home state of deceased soldiers render increases in local soldier fatalities unpredictable and exogenous to the firm and greatly diminish the threat of a correlated omitted variable. Another threat to inference, reverse causality, is also unlikely because it is implausible that discretionary accruals at a defense firm will cause fatalities of soldiers that tend to live near defense firm headquarters. In addition, we address self-selection concerns by identifying treatment defense firms in the pre-war year of 2000. Defense firms do not enter and exit the sample after the commencement of war. To address concerns regarding covariate balance across treatment and control samples, we replicate all tests using 1) a performance-matched sample, 2) an entropy-balanced sample, and 3) a defense firm-only sample. These tests address concerns regarding inherent differences between the defense and non-defense firms comprising our main sample.

We point to a corroborating insight from coefficient estimates reported in Table 5. Income-decreasing accruals, if recorded to reduce political costs, should reverse when the threat of political costs dissipates. We adapt the research design prescribed by Dechow et al. (2012) to our setting and include in our estimation of Equation (1) a variable that captures lagged variation in soldier fatalities to capture this reversal. We expect discretionary accruals recorded in t to reverse in $t+1$ because Koch and Nicholson (2016) and Althaus et al. (2012) show that the effect of local soldier fatalities on political outcomes lasts no longer than one year. The coefficient on the variable *Lagged Local Soldier Fatalities* × *Defense Firm* supports this prediction. *Lagged Local Soldier Fatalities* is equal to soldier fatalities per capita by state-year for $t-1$. The coefficient is positive, statistically significant, and 44% of the magnitude of the Column 4 coefficient for the test variable, *Local Soldier Fatalities* × *Defense Firm,* indicating that a significant portion of the income-decreasing discretionary accruals recorded around local soldier fatalities reverses as the threat of political costs dissipates. For the remainder of our tests, we focus on the specification used in Column 4 of Table 5, which employs firm and industry × year fixed effects and our suite of control variables.

7.2. Construct validity

We redefine our soldier fatality measure to determine if our results vary predictably. We expect defense firms' income-decreasing accruals around local soldier fatalities to increase with the vulnerability of the defense firm to political costs. Vulnerability to political costs increases in military revenue as a proportion of total sales. We use the defense sales procurement ratio as a proxy for vulnerability to political costs. In Table 6, we vary the threshold of this ratio in one percent increments from 0 to 5 percent to determine if firms with more U.S. military revenues record more income-decreasing accruals (our main tests use a 5 percent threshold). Samuels (2020); Mills et al. (2013) and Karpoff et al. (1999) employ similar construct validity tests in the federal government procurement setting.

[28] We also note that the coefficient estimate on National Soldier Fatalities, a variable defined as the logarithm of the annual count of soldier fatalities, is negative and statistically significant in Column 2 (year fixed effects subsume this variable in Columns 3 and 4). This coefficient at first appears to suggest that defense firms record income-decreasing accruals when the number of nationwide soldier fatalities is higher, but there are several important obstacles to this interpretation. We do not use nationwide fatalities to test the political cost hypothesis because nationwide fatalities could plausibly affect economic determinants of defense firms' accruals that are unrelated to the threat of increased political costs. Larson (1996, p. 53) describes “two bits of conflicting conventional wisdom” that suggest a relation between nationwide fatalities and defense firm operating performance. First, the public may demand immediate withdrawal as U.S. casualties mount. Second, casualties may lead to demands from an inflamed public “for escalation to a decisive victory.” Consequently, accruals around nationwide fatalities could be explained instead by 1) managers recording accruals in anticipation of fewer procurement contracts in the expectation that fatalities will lead to a quicker cessation of war or, inversely, 2) managers recording accruals in anticipation of more procurement contracts in the expectation that more fatalities will lead to an increase in war activity. Overall, because of these confounds (which mirror confounds in the prior political cost literature), we cannot draw credible inferences about the political cost hypothesis by examining nationwide fatalities. We face none of these confounds when examining the local setting.

Table 6
Further evidence from variation in defense firms' defense procurement sales ratios.

		(1)	(2)	(3)	(4)	(5)	(6)
Defense Firm Procurement Sales Ratio Threshold:		>0%	>1%	>2%	>3%	>4%	>5%
	Pred.	Discretionary Accruals					
Local Soldier Fatalities × Defense Firm	−	**−0.00341****	**−0.00819*****	**−0.00890*****	**−0.0103*****	**−0.0103*****	**−0.0110*****
		(-1.99)	**(-2.76)**	**(-2.46)**	**(-2.49)**	**(-2.39)**	**(-2.36)**
Local Soldier Fatalities		0.000580	0.000462	0.000383	0.000368	0.000352	0.000338
		(0.71)	(0.61)	(0.51)	(0.50)	(0.48)	(0.46)
Lagged Local Soldier Fatalities		−0.00155*	−0.00138*	−0.00132*	−0.00131*	−0.00131*	−0.00132*
		(-1.86)	(-1.72)	(-1.66)	(-1.66)	(-1.66)	(-1.67)
Lagged Local Soldier Fatalities × Defense Firm	+	**0.00213****	**0.00315****	**0.00341***	**0.00408***	**0.00437****	**0.00486****
		(1.98)	**(1.79)**	**(1.53)**	**(1.59)**	**(1.65)**	**(1.74)**
National Soldier Fatalities × Defense Firm		0.00449	0.00751	0.00622	0.00873	0.00683	0.00738
		(1.05)	(1.11)	(0.79)	(1.04)	(0.77)	(0.80)
Constant		−0.0525***	−0.0502***	−0.0491***	−0.0496***	−0.0490***	−0.0488***
		(-2.84)	(-2.78)	(-2.72)	(-2.74)	(-2.71)	(-2.71)
R-Squared		0.488	0.488	0.488	0.488	0.488	0.488
Number of Observations		16,749	16,749	16,749	16,749	16,749	16,749
Standard Errors Clustered By:		Firm	Firm	Firm	Firm	Firm	Firm
Table 5 Control Variables:		Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects:		Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year Fixed Effects:		Yes	Yes	Yes	Yes	Yes	Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. We repeat the analysis in Table 5 after replacing our treatment group with Defense Firms with procurement sales ratios above the percentage specified in each column header. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

We report results after defining the treatment group to include firms with a procurement sales ratio greater than 0%, 1%, 2%, 3%, 4% and 5% in Columns 1 to 6, respectively. The sample size of the treatment group predictably shrinks as we increase the procurement sales ratio threshold. At greater than 0 percent, the treatment group numbers 2823 firm-years and 398 firms. At 5 percent, the treatment group numbers 582 firm-years and 77 firms. We find predictable variation with the magnitude of discretionary accruals around local soldier fatalities monotonically increasing as we exclude defense firms with less vulnerability to political costs.

We tabulate results with the treatment group including defense firms with at least one defense contract in Column 1 of Table 6. The statistically significant coefficient estimate on the test variable, *Local Soldier Fatalities × Defense Firm,* is −0.00341. This coefficient is approximately one-third the size of the coefficient reported in Column 6 of Table 6 where we include in the treatment group only those firms with military revenues that comprise at least 5 percent of total sales. These results show that defense firms more vulnerable to political costs record more income-decreasing accruals and that this relationship varies predictably and monotonically. The results are also economically meaningful. A one standard deviation increase in local soldier fatalities (1.06) causes an increase in income-decreasing total accruals equal to −0.36 percent of total assets in Column 1 and income-decreasing accruals equal to −1.17 percent in Column 6. We also note that firms with greater vulnerability to political costs record larger accruals reversals in the year subsequent to increases in local soldier fatalities.

7.3. Cross-sectional tests

7.3.1. State characteristics

We expect firm responses to vary predictably in the cross-section of state characteristics. Miller and Albert (2015), who test the “if it bleeds, it leads” hypothesis of news coverage (Hackett, 1989; Van Belle, 2000), motivate our first cross-sectional test. Miller and Albert (2015) find that broadcasters local to the permanent residence of a deceased soldier provide more news coverage of local soldier fatalities. For example, Gartner (2004) finds more media coverage of the 2000 bombing of the USS Cole in states in which the deceased soldiers resided. Gartner et al. (1997, p. 675) write that “even (a local) who has no direct connection to a battle casualty is more likely to read about local boys lost in the war in local newspapers, to hear discussion of such matters in the local tavern, and to learn the announcements of deaths from the church pulpit.” Simultaneously, local media provides greater coverage of local firms and increases awareness of local firms among the local public (e.g., Engelberg and Parsons, 2011; Gurun and Butler, 2012). Overall, we expect media coverage of soldier fatalities to increase the threat of political costs because higher levels of local media coverage are likely to foment public outrage with respect to war costs and war profiteering.

Table 7
Defense firms' earnings management around local soldier fatalities, test of the moderating effects of media coverage.

	Pred.	(1) Discretionary Accruals
Local Soldier Fatalities × Defense Firm		−0.000427
		(-0.06)
Local Soldier Fatalities × Defense Firm × Local Fatality Media Coverage	−	**−0.00702***
		(-1.58)
Local Fatality Media Coverage		0.000154
		(0.06)
Local Soldier Fatalities × Local Fatality Media Coverage		−0.000553
		(-0.50)
Defense Firm × Local Fatality Media Coverage		0.0222**
		(2.04)
Local Soldier Fatalities		0.00118
		(0.67)
National Soldier Fatalities × Defense Firm		0.00277
		(0.29)
Lagged Local Soldier Fatalities		−0.00139*
		(-1.76)
Lagged Local Soldier Fatalities × Defense Firm	+	**0.00576****
		(1.95)
Constant		−0.0494***
		(-2.66)
R-Squared		0.488
Number of Observations		16,749
Standard Errors Clustered By:		Firm
Table 5 Control Variables:		Yes
Firm Fixed Effects:		Yes
Industry × Year Fixed Effects:		Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Local Fatality Media Coverage is a variable equal to one if the state-year has above median media coverage of local soldier fatalities and zero otherwise. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

We adapt to our setting the method developed by Baker et al. (2016) to generate our measure of media coverage.[29] We calculate our bad news war article index as the number of articles referring to soldier fatalities published within a state and year, divide by the total number of war-related articles in a year, and then divide again by the number of articles published in the state-year. We collect the number of articles mentioning "Iraq" or "Afghanistan" and word stems for synonyms of "fatality" for every state-year. We also gather the number of articles that make any reference to "Iraq" or "Afghanistan" and consider these war-related articles. We divide the number of fatality-related articles by the number of war articles because a raw count could otherwise reflect more media coverage of the war in general and not soldier fatalities specifically. We then divide by the total number of articles published in a year to create an index comparable across state-years. We identify 284,894 articles that meet our definition of fatality-related articles, 818,441 that meet our definition of war-related articles, and 56,063,761 published in total during the sample period. We partition the index into state-years with above- and below-median values.

Table 7 reports results from the estimation of Equation (1) after adding variables capturing the interaction between our test variable, *Local Soldier Fatalities* × *Defense Firm*, and our fatality media coverage, *Local Fatality Media Coverage*. We also add all main effects and subordinate interactions (Aiken and West, 1991).

The test variable, *Local Soldier Fatalities* × *Defense Firm* × *Local Fatality Media Coverage,* captures the difference in the effect of local soldier fatalities on defense firms' discretionary accruals when defense firms are located in states with high versus low levels of media coverage of local soldier fatalities. Consistent with our prediction that defense firms record more income-decreasing accruals in response to increased local soldier fatalities when the amount of media coverage of local soldier fatalities is high, the coefficient estimate on the test variable, *Local Soldier Fatalities* × *Defense Firm* × *Local Fatality Media Coverage*, is negative and statistically significant. These results suggest that firms vary their behavior predictably when the

[29] Baker et al. (2016) perform a count of the frequency of articles in the ten leading U.S. newspapers with terms related to economic policy uncertainty and relate their time-varying measure to stock price volatility, investment and employment for firms in politically sensitive sectors. We replicate their method with the exception that we count the frequency of articles referring to soldier fatalities from all sources available in Factiva rather than only articles in the ten leading U.S. newspapers. Gulen and Ion (2015) also use this method.

threat of political costs varies across state and year and further corroborate our main inference that managers record accruals to reduce the threat of political costs in response to increases in visibility.[30]

7.3.2. Firm characteristics

We next examine how defense firms' political strength vis-à-vis the U.S. government moderates the effect of political costs on accrual decisions. If defense firms with more market power vis-à-vis the government are politically stronger and less vulnerable to political costs, then the incentive to engage in earnings management to reduce political costs will be weaker. Defense firms can produce both homogenous goods (e.g., textiles [uniforms, footwear], construction [buildings and earthworks], logistics [foodstuffs, catering]), for which there is a competitive market, and proprietary technologies, for which the pool of suppliers is shallow. For example, during our sample period, nine public companies fulfilled 197 contracts for Complete Guided Missile Systems (Product and Service Code [PSC] 1425) with 1.1 bids per contract on average. Meanwhile, 43 public companies fulfilled 6248 contracts for Textile Fabrics (PSC: 8305) with 2.3 bids per contract on average. Moreover, systems underlying advanced technologies are often proprietary and unusable by competing vendors, entailing high DOD switching costs for advanced technology vendors (DOD, 2013). We expect the DOD to be more reliant on suppliers of advanced weapons technology because of few suppliers and high switching costs and, consequently, for these suppliers to be less susceptible to the political costs of reporting high profits. We posit that these defense firms are less likely to record discretionary accruals to reduce the threat of political costs because these firms can more easily pass new costs related to political costs (e.g., excess-profit taxes) on to the government through higher prices. For example, in the months leading up to the adoption of a munitions tax in 1916, manufacturers began including in contracts that the cost of the new tax would be borne by the buyer (Wall Street Journal, 1916).

Overall, we expect the relationship between political costs and discretionary accruals to be attenuated for advanced weapon suppliers. To test this cross-sectional prediction, we closely examine the details of procurement contracts to identify those defense firms that supply advanced weapon technologies, and we construct a new variable equal to one when a firm provisions advanced weapon technologies to the DOD in a firm-year and zero otherwise.[31]

Column 1 of Table 8 reports the results of estimating Equation (1) after including interaction variables between our weapon supplier variable (*Test Variable*) and *Local Soldier Fatalities* × *Defense Firm*. In this model, *Local Soldier Fatalities* × *Defense Firm* × *Test Variable* captures the difference in the impact of soldier fatalities on defense firms' discretionary accruals when the defense firm provisions the DOD with advanced weapon systems rather than more homogenous goods and services. Consistent with the notion that those firms with more market power and political strength are less vulnerable to political costs, the coefficient on *Local Soldier Fatalities* × *Defense Firm* × *Test Variable* is positive and statistically significant. Overall, advanced weapon suppliers with greater political strength appear less susceptible to political costs vis-a-vis firms supplying homogenous goods, suggesting that political strength weakens the relationship between political costs and accounting choices.

Our final cross-sectional investigation examines defense firms with foreign sales. The presence of more foreign customers reduces firm reliance on sales to the U.S. government. These firms have a weaker incentive to engage in earnings management to reduce political costs because these firms can more easily exit the U.S. market if the government imposes onerous political costs on U.S. defense firms. These firms will be less susceptible to political costs because supply chains already exist through which to push production in the event the U.S. market loses viability. We draw foreign sales data from Compustat. To examine this prediction, we construct a measure of foreign sales that is equal to one when firms' foreign sales divided by total sales are above the sample median and zero otherwise.[32]

Column 2 of Table 8 reports the results of an estimation of Equation (1) after including interaction variables between our foreign sales variable and *Local Soldier Fatalities* × *Defense Firm*. In this model, *Local Soldier Fatalities* × *Defense Firm* × *Test Variable* captures the difference in impact of soldier fatalities on defense firms' discretionary accruals when the defense firms' foreign sales are high. The coefficient on *Local Soldier Fatalities* × *Defense Firm* × *Test Variable* is positive and statistically significant. This suggests that firms with less dependence on the U.S. government are less vulnerable to political costs. Overall, tests tabulated in Table 8 show how evidence in support of the political cost hypothesis varies predictably with the market power and political strength of defense firms and lends additional credibility to the power of the political cost hypothesis to explain defense firms' accounting choices.

[30] In untabulated analyses, we examine whether local variation in media coverage alone is associated with defense firms' accounting choices. We find no association between local media coverage and accounting choices in accrual models that do not account for local soldier fatalities.

[31] Specifically, we consider firms provisioning the following products as advanced weapon system suppliers: nuclear ordnance, weapon control equipment, ammunitions and explosives, guided missiles, aircraft and airframe structural components, aircraft components and accessories, aircraft launching landings and ground handling equipment, space vehicles.

[32] The partitioning variables that split firms into below- and above-median foreign sales and firms with and without advanced weaponry have a low correlation of 0.0774. Our sample of 77 defense firms includes 33 that provide advanced weaponry to the DOD. Of this 33, 14 have above-median foreign sales, and 19 have below-median foreign sales. We also find that our partitioning variable capturing states with above- and below-median media coverage of local soldier fatalities is not highly related to either partitioning variable capturing foreign sales and provision of advanced weaponry. The correlations are 0.034 and −0.0079, respectively.

Table 8
Defense firms' earnings management around local soldier fatalities, tests of the moderating effects of defense firms' market power.

Test variable:	Pred.	(1) Advanced Weapon Suppliers	(2) Foreign Sales
		Discretionary Accruals	
Local Soldier Fatalities × Defense Firm		−0.0175***	−0.0292***
		(-2.85)	(-2.86)
Local Soldier Fatalities × Defense Firm × Test Variable	+	**0.0136*****	**0.0128*****
		(2.55)	**(2.57)**
Test Variable		−0.00568	0.00309
		(-1.26)	(1.16)
Local Soldier Fatalities × Test Variable		0.00137	−0.000433
		(0.84)	(-0.43)
Defense Firm × Test Variable		−0.0410***	−0.0231
		(-2.87)	(-1.55)
Local Soldier Fatalities		0.000285	0.000946
		(0.38)	(0.55)
National Soldier Fatalities × Defense Firm		0.00557	0.00571
		(0.60)	(0.67)
Lagged Local Soldier Fatalities		−0.00133*	−0.00132*
		(-1.68)	(-1.68)
Lagged Local Soldier Fatalities × Defense Firm	+	**0.00568****	**0.00465***
		(2.00)	**(1.63)**
Constant		−0.0476***	−0.0509***
		(-2.63)	(-2.75)
R-Squared		0.488	0.488
Number of Observations		16,749	16,749
Standard Errors Clustered By:		Firm	Firm
Table 5 Control Variables:		Yes	Yes
Firm Fixed Effects:		Yes	Yes
Industry × Year Fixed Effects:		Yes	Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Test Variable is defined in the column header. Weapon Suppliers is an indicator variable equal to one if the firm supplied weapon technologies to the U.S. government and zero otherwise. Foreign sales is an indicator variable equal to one if the firm had above median foreign sales in the year and zero otherwise. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

7.4. Robustness tests

7.4.1. Placebo tests

This section reports results for four placebo tests. We report results for four tests in Table 9. We first show that we cannot replicate our results when we examine firms that should not be affected by the public awareness of the war effort. The first placebo test redefines *Defense Firm* as an indicator variable equal to one for federal government procurement firms uninvolved in the defense industry. This test addresses the potential concern that accrual choices of all federal government procurement firms vary with soldier fatalities for some unobserved reason.

We estimate Equation (1) using this placebo definition and present the results in Column 1 of Table 9. Our redefined test variable, *Local Soldier Fatalities* × *Defense Firm,* is statistically insignificant. The results of the placebo test confirm that non-DOD government contractors do not alter their discretionary accruals in response to soldier fatalities. This test fails to show an association between accrual choices of non-DOD federal government contractors and rules out the concern that accrual choices of all federal government procurement firms vary with soldier fatalities for some unobserved reason.

Second, we redefine our key measure by changing the home state coded for each recently deceased soldier. Instead of examining firms in the permanent home state of recently deceased soldiers, we examine firms in the state in which the soldier is based before deployment to Afghanistan or Iraq. We draw these data from www.icasualties.org. While the permanent soldier residence state is the relevant locale according to the *proximate casualties hypothesis*, the state in which the soldier was stationed before deployment is a second potential locale we could have considered for our main tests. However, we do not expect the state in which the soldier was stationed before deployment to generate as much local awareness of the war as the soldier's permanent home state. This is because soldiers are unlikely to make as many civilian connections in the pre-deployment base state as in the permanent home state and because long-term community and family connections to the soldier's permanent home state are expected to be stronger and more numerous than in the pre-deployment base state.

We estimate Equation (1) with our redefined state-year fatalities measure and present the results in Column 2 of Table 9. Our results validate our expectation as we find that local defense firms' discretionary accruals are insensitive to soldier

Table 9
Placebo tests.

	(1)	(2)	(3)	(4)
	Non-Defense Procurement Firm Placebo	Military Base State Placebo	State-Year Police Fatalities Placebo	State-Year Veteran Suicides Placebo
Local Soldier Fatalities × Defense Firm	**−0.00153**	**0.00454**	**−0.000157**	**0.0000462**
	(-0.97)	**(1.52)**	**(-0.02)**	**(0.11)**
Local Soldier Fatalities	0.000292	0.000392	−0.00192	0.0000237
	(0.34)	(0.70)	(-0.97)	(0.19)
Lagged Local Soldier Fatalities	−0.00137	0.000478	0.000425	0.0000782
	(-1.63)	(0.83)	(0.19)	(0.70)
Lagged Local Soldier Fatalities × Defense Firm	**0.000991**	**0.00149**	**−0.00451**	**−0.0000407**
	(0.99)	**(0.55)**	**(-0.58)**	**(-0.09)**
National Soldier Fatalities × Defense Firm	0.00334	−0.00614	−0.00952	−0.0103
	(0.83)	(-1.00)	(-1.50)	(-1.21)
Constant	−0.0516***	−0.0483***	−0.0466***	−0.0484***
	(-2.75)	(-2.71)	(-2.60)	(-2.68)
R-Squared	0.487	0.488	0.487	0.487
Number of Observations	16,749	16,749	16,749	16,749
Standard Errors Clustered By:	Firm	Firm	Firm	Firm
Table 5 Control Variables:	Yes	Yes	Yes	Yes
Firm Fixed Effects:	Yes	Yes	Yes	Yes
Industry × Year Fixed Effects:	Yes	Yes	Yes	Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics. In column 1, Defense Firm is a variable equal to one for government procurement firms uninvolved in the defense industry. In columns 2, 3 and 4, Defense Firm is defined as in the main tests. In column 1, Local Soldier Fatalities is defined as in the main tests. In column 2, Local Soldier Fatalities are defined using the local per capita number of soldiers who perish where local is defined using the state of military base station prior to deployment rather than the permanent residence. In column 3, Local Soldier Fatalities is equal to the per capita number of police officers killed in a state-year. In column 4, Local Soldier Fatalities is equal to the per capita number of veteran suicides in a state-year. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

fatalities when fatalities are defined as local to firms when firms are headquartered in the state in which the soldier was stationed immediately prior to deployment. These results are consistent with the notion that defense firms that are not local to the deceased soldiers' permanent home state are not subject to the same degree of public awareness as defense firms headquartered in the permanent home state of the deceased soldier.

Third, we examine defense firms' accrual response to local variation in police fatalities. This is an appropriate placebo group because police fatalities generate a large media response but one that should be orthogonal to defense firm financial reporting choices. We draw police fatality data from the Officer Down Memorial Page.[33] We estimate Equation (1) with our redefined fatalities measure and report the results in Column 3 of Table 9. We find no relationship between local defense firm accruals and local police fatalities.

Our fourth placebo test examines local soldier fatalities that do not cause variation in the public awareness of the human cost of war. We examine defense firms' accrual response to state-year variation in the 49,207 veteran suicides that occur during our sample period. This is an appropriate placebo group because, veteran suicides that occur at more than double the rate of non-veteran suicides, are a plausible human cost of war but do not generate media visibility and so should not increase public awareness of the human cost of war. Veteran suicide data are drawn from individual state health and vital records departments and the Center for Disease Control.[34] We estimate Equation (1) with our redefined state-year fatalities measure and report the results in Column 4 of Table 9. We find no relationship between local defense firm accruals and local veteran suicides. Overall, these results suggest that our tests are not predisposed to generating spurious associations between defense firms' discretionary accruals and placebo sources of state-year variation.

7.4.2. Defense firms' State(s) of operation

Using defense firms' headquarter states to identify local soldier fatalities introduces measurement error to our variable of interest if firm operations occur in other states. It is possible that a firm has major operations outside the headquarter state and that variation in local soldier fatalities in these non-headquarter states may also spur public awareness of the defense firms' profits. We will observe a test variable coefficient estimate biased toward zero if this is the case because the effect of local and non-local soldier fatalities on local defense firms' accrual decisions would be similar.

We take steps to address this concern by redefining firm location as the location of actual firm operations using data obtained from Garcia and Norli (2012). Garcia and Norli (2012) count state names from firms' 10-Ks filed with the SEC to proxy

[33] Data drawn from https://www.odmp.org/search/browse.

[34] Data from these sources are compiled at https://backhome.news21.com/interactive/suicide-interactive/.

Table 10
Defense firms' earnings management around local soldier fatalities, defining firm location by operations rather than headquarters.

	Pred.	(1) Firm location defined using headquarter state (mirroring main test in Column 4 of Table 5)	(2) Firm location defined using location of largest operations	(3) Firm location defined using location of all operations (firm-year-state panel)
Local Soldier Fatalities × Defense Firm	−	**−0.0110*** **	**−0.0106** **	**−0.00669** **
		(-2.36)	**(-2.22)**	**(-1.74)**
Local Soldier Fatalities		0.000338	−0.000260	−0.000272
		(0.46)	(-0.34)	(-0.29)
Lagged Local Soldier Fatalities		−0.00132*	−0.00139*	−0.000776
		(-1.67)	(-1.73)	(-0.84)
Lagged Local Soldier Fatalities × Defense Firm	+	**0.00486** **	**0.00485** **	**0.00491** **
		(1.74)	**(1.69)**	**(2.10)**
National Soldier Fatalities × Defense Firm		0.00738	0.00666	0.00340
		(0.80)	(0.71)	(0.42)
Constant		−0.0488***	−0.0490***	−0.0375*
		(-2.71)	(-2.68)	(-1.85)
R-Squared		0.488	0.496	0.499
Number of Observations		16,749	15,631	134,106
Standard Errors Clustered By:		Firm	Firm	Firm
Table 5 Control Variables:		Yes	Yes	Yes
Firm Fixed Effects:		Yes	Yes	Yes
Industry × Year Fixed Effects:		Yes	Yes	Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

for the potential states in which a firm has operations. Firms provide information on their factories, warehouses and sales offices in their 10-Ks. The authors construct a measure for firms' geographic dispersion as the number of different states mentioned in sections “Item 1: Business,” “Item 2: Properties,” “Item 6: Consolidated Financial Data,” and “Item 7: Management's Discussion and Analysis” of the 10-K filing. Because the data are available from 1994 to 2008, we use the 2008 dispersion data for years after 2008.

Using the geographic location of firm operations, we perform two robustness tests to determine whether our results vary when we redefine the location of the firm to capture the location of firm operations rather than firm headquarters. First, we redefine firm location as the state in which firms have the greatest proportion of operations. Our expectation is that our results should be robust to this change because, using the Garcia and Norli (2012) data, we find that 62% of firms have their largest operations in their headquarter state. We tabulate the results in Table 10. The first column forms the benchmark, replicating our main result presented in Column 4 of Table 5. The second column examines our result when we instead use the state in which the firm has the largest operations as the anchor state for the firm. Our results are similar after redefining firm state location as the state in which firms conduct most operations. Second, we examine whether the effect we observe is detectable when we include all states in which the firm has operations. This test transforms our firm-year panel into a firm-year-state panel because we now include firm-year observations for each state in which the firm has operations.[35] We tabulate the results in Column 3 of Table 10. Using this panel, we show that our results are similar after including all states in which the firm has operations.

7.4.3. *Defense firms' geographic clustering*

We note geographic clustering by defense firms in our sample. California and New York are home to one-third of defense firms. Firm fixed effects capture much of the variation in accruals owing to time-invariant state-related factors. Nonetheless, given the proportion of defense firms from these two states, we repeat our analysis after removing California and New York and report the results in Columns 1 and 2 of Table 11, respectively. Our results are consistent with our main results reported in Table 5. In untabulated analyses, we draw identical inferences when we re-examine our main tests after we remove any state from our sample.

7.4.4. *Removal of recession years from sample period*

We address concerns that our results are spuriously generated by unusual accrual behavior during the financial recession of 2008–2009 with the use of year fixed effects. Nonetheless, we further address potential concerns by removing these years

[35] Accordingly, the number of observations increases from 16,749 in Column 2 to 134,106 in Column 3 of Table 10.

Table 11
Defense firms' earnings management around local soldier fatalities, robustness tests.

	Pred.	(1)	(2)	(3)
		State-Year Tests without California	State-Year Tests without New York	State-Year Tests Absent Recession Years
Local Soldier Fatalities × Defense Firm	−	**−0.00798****	**−0.0117*****	**−0.0133*****
		(-1.65)	**(-2.41)**	**(-2.70)**
Local Soldier Fatalities		0.000525	0.000407	0.000696
		(0.69)	(0.54)	(0.84)
Lagged Local Soldier Fatalities		−0.00116	−0.00132	−0.00187**
		(-1.45)	(-1.63)	(-2.00)
Lagged Local Soldier Fatalities × Defense Firm	+	**0.00427***	**0.00522****	**0.00670****
		(1.45)	**(1.83)**	**(1.82)**
National Soldier Fatalities × Defense Firm		0.00138	0.0111	0.00785
		(0.14)	(1.09)	(0.73)
Constant		−0.0392**	−0.0502***	−0.0417**
		(-1.97)	(-2.67)	(-2.20)
R-Squared		0.497	0.483	0.522
Number of Observations		13,544	15,324	13,607
Standard Errors Clustered By:		Firm	Firm	Firm
Table 5 Control Variables:		Yes	Yes	Yes
Firm Fixed Effects:		Yes	Yes	Yes
Industry × Year Fixed Effects:		Yes	Yes	Yes

This table reports results from regressions of discretionary accruals on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

from our sample and replicating our main test. We report the result in Column 3 of Table 11. These results are consistent with our main result reported in Column 4 of Table 5. In untabulated analyses, we discern our results are also insensitive to excluding any year from the sample.

7.4.5. Alternate discretionary accrual models

Our findings are also robust to seven alternative discretionary accrual models: the residual from the original Jones (1991) model; the modified Jones model (Dechow et al., 1995); the modified Jones model including lead, lagged and contemporaneous cash flows (Dechow and Dichev, 2002; McNichols, 2002); the Jones (1991) model with controls for economics gains and losses (Ball and Shivakumar, 2006); the performance-adjusted modified Jones (1991) model with firm and year fixed effects (Kothari et al., 2016); a model including all parameters listed in the foregoing models; and a one-step model that addresses concerns raised by Chen et al. (2018) regarding two-step discretionary accrual models.[36] These results are available upon request.

7.4.6. Real actions-based earnings management test

Evidence that firms engage in real actions to lower earnings in our setting would corroborate our main findings. These real actions are an alternate, if costlier, method by which firms can orchestrate the appearance of lower profits (Badertscher, 2011; Cohen et al., 2008; Zang, 2012). To our knowledge, these are the first tests of the political cost hypothesis using real actions-based earnings management models.

Columns 1 to 4 of Table 12 examine various expense accounts including sales, general and administrative expenses (SG&A; Column 1); production costs (Column 2); advertising expenses (Column 3); and research and development expenses (R&D; Column 4). These models emulate the prior literature (e.g., Roychowdhury, 2006; Cohen et al., 2008; Cohen and Zarowin, 2010). We scale SG&A, advertising and R&D expenses by lagged total assets and multiply by −1. We reverse the sign of these variables so that coefficients generated conform with the previous interpretation that negative coefficients connote income-decreasing effects. Production costs are measured as the sum of cost of goods sold plus the annual change in inventory divided by lagged assets, and an increase (decrease) in production costs has the effect of decreasing (increasing) cost of goods sold reported on the income statement and increasing (decreasing) net income.

As in our main tests, the test variable is *Local Soldier Fatalities × Defense Firm.* The coefficient on this variable is negative and statistically significant in each column of Table 12. These results provide evidence that, concurrent with efforts to reduce political costs by recording income-decreasing accruals, defense firms simultaneously incur additional discretionary expenses. Unlike accruals, we would not expect discretionary expenses to reverse once the threat of political costs dissipates. As

[36] The correlations between these seven measures of discretionary accruals range from 0.6969 to 0.9899.

Table 12
Defense firms' real actions based earnings management around local soldier fatalities.

	Pred.	(1)	(2)	(3)	(4)
		Sales, General and Administration Expense	Production Costs	Advertising Expense	Research and Development Expenditures
Local Soldier Fatalities × Defense Firm	−	**−0.00771****	**−0.0113****	**−0.000577****	**−0.00458***
		(-2.13)	**(-1.75)**	**(-1.73)**	**(-1.46)**
Local Soldier Fatalities		0.00252*	−0.00147	0.000241	0.000911
		(1.77)	(-0.62)	(1.21)	(0.86)
Lagged Local Soldier Fatalities		0.00215*	−0.000688	0.000183	0.000706
		(1.83)	(-0.31)	(0.94)	(1.00)
Lagged Local Soldier Fatalities × Defense Firm	**Ø**	**0.00340**	**−0.00549**	**0.000116**	**0.00142**
		(1.04)	**(-0.90)**	**(0.37)**	**(0.75)**
National Soldier Fatalities × Defense Firm		−0.00235	0.0628**	0.00157	0.0166
		(-0.15)	(2.40)	(1.54)	(1.39)
1/Total Assets		−1.816***	2.580***	0.0547*	0.683***
		(-4.39)	(4.72)	(1.73)	(2.76)
ΔRevenue - ΔAccounts Receivable		−0.0842***	0.496***	−0.000243	−0.0129
		(-2.82)	(6.74)	(-0.10)	(-1.04)
Property, Plant and Equipment, Gross		−0.221***	0.302***	0.00118	0.0307**
		(-8.27)	(8.23)	(0.43)	(2.58)
Return on Assets		0.180***	−0.522***	0.00994***	0.0789***
		(4.34)	(-9.54)	(3.32)	(2.74)
Cash Flows$_{t-2}$		0.00624	0.0476**	0.00578**	0.0139
		(0.34)	(2.03)	(2.16)	(0.88)
Cash Flows$_{t-1}$		0.0192	0.0625**	0.000169	0.178***
		(0.92)	(2.28)	(0.06)	(7.42)
Cash Flows$_t$		0.0318	1.022***	−0.00380	0.196
		(0.10)	(3.50)	(-0.31)	(0.79)
Cash Flows$_{t+1}$		0.0164	0.0950***	0.00576*	0.0348*
		(0.65)	(4.35)	(1.72)	(1.91)
Cash Flows$_{t+2}$		−0.0278	−0.00306	−0.00362	0.00617
		(-1.04)	(-0.13)	(-1.07)	(0.34)
Size		−0.00119	0.0727***	−0.00208	0.0486***
		(-0.17)	(6.08)	(-1.34)	(7.43)
Book Leverage		0.0416**	−0.135***	0.00372	0.0286
		(1.98)	(-4.23)	(1.60)	(1.58)
Sales Growth		−0.0614***	−0.0453*	−0.00125	0.00761
		(-4.17)	(-1.81)	(-0.56)	(0.49)
Change in Employees		0.0419***	−0.104***	0.00109	−0.00548
		(5.94)	(-7.26)	(0.83)	(-0.95)
Std (Sales)		−0.0324	−0.0134	−0.00717*	0.00229
		(-1.48)	(-0.36)	(-1.72)	(0.23)
NOA$_{t-1}$		0.0121*	0.00963	−0.000100	0.00401
		(1.87)	(1.50)	(-0.24)	(0.72)
Market Return		−0.00109	−0.0121***	−0.0000156	−0.000520
		(-0.38)	(-3.91)	(-0.05)	(-0.21)
Market-to-Book Ratio		0.000685	−0.000405	−0.000133	0.000674
		(0.72)	(-0.53)	(-1.56)	(1.04)
Loss		−0.0225***	0.0431***	0.00000397	−0.00654**
		(-5.70)	(7.30)	(0.01)	(-2.06)
Operating Cycle		0.0000711	0.000763***	−0.0000151*	−0.000191**
		(0.87)	(6.81)	(-1.79)	(-2.07)
Stock Volatility		0.00165	−0.00339	−0.000265	0.0000597
		(0.70)	(-1.24)	(-0.33)	(0.05)
Standard Deviation of Earnings		−0.0671**	0.0649**	−0.000653	−0.0343*
		(-2.48)	(2.06)	(-0.25)	(-1.66)
Standard Deviation of Cash Flows		0.00596	−0.109**	−0.00578	−0.0962***
		(0.12)	(-2.04)	(-0.78)	(-2.74)
Industry Adjusted Cash Flows (A)		−0.150	−0.629***	0.0104	−0.134
		(-0.59)	(-2.59)	(0.87)	(-0.65)
Negative Industry Adjusted Cash Flows (B)		0.00167	−0.00265	0.000508	0.00682**
		(0.34)	(-0.47)	(1.06)	(2.45)
(A) × (B)		0.278	−0.570***	0.000200	−0.140
		(1.51)	(-4.30)	(0.07)	(-1.49)
Idiosyncratic Shocks		−0.0660	0.159*	0.0207**	0.0596
		(-1.02)	(1.67)	(2.07)	(1.10)
Revenue$_{t-1}$/Total Assets$_{t-1}$		−0.106***	0.326***	−0.00239	−0.0233**
		(-3.55)	(3.58)	(-1.19)	(-2.10)
Revenue/Total Assets$_{t-1}$		0.0276	0.0844	−0.0114***	−0.00806
		(1.29)	(1.32)	(-3.55)	(-0.87)

Table 12 (*continued*)

	Pred.	(1) Sales, General and Administration Expense	(2) Production Costs	(3) Advertising Expense	(4) Research and Development Expenditures
ΔRevenue$_{[(t)-(t-1)]}$/Total Assets$_{t-1}$		0.000314	0.00478	−0.000165	−0.0160**
		(0.16)	(0.66)	(-0.46)	(-2.11)
ΔRevenue$_{[(t-1)-(t-2)]}$/Total Assets$_{t-1}$		−0.0756	0.483	−0.0129	−0.180
		(-0.33)	(1.10)	(-1.04)	(-1.41)
Constant		−0.0666	−0.414***	0.0126	−0.384***
		(−1.04)	(-3.94)	(0.98)	(−8.41)
R-Squared		0.873	0.953	0.868	0.803
Number of Observations		16,749	16,749	16,749	16,749
Standard Errors Clustered By:		Firm	Firm	Firm	Firm
Firm Fixed Effects:		Yes	Yes	Yes	Yes
Industry × Year Fixed Effects:		Yes	Yes	Yes	Yes

This table reports results from regressions of income statement expenses on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, one-tailed where we have predictions, two-tailed otherwise.

noted by Ernstberger et al. (2017, p. 38), "cuts in R&D during one quarter are often hard to reverse given internal budget constraints … price discounts in a certain period may also be hard to reverse in the next period due to altered customer expectations." Consistent with this expectation, and distinct from discretionary accruals, we find no effect of local soldier fatalities on defense firm discretionary expenses in $t + 1$. Overall, our real actions-based earnings management evidence corroborates the inferences we draw from our accruals-based earnings management tests.

7.4.7. *Alternate sample #1: performance-matched sample*

Differences in performance across treatment and control firms can give rise to the spurious appearance of discretionary accruals. While the performance of defense firms and our control group is similar during our sample period, and we explicitly control for performance in the first and second stages of our accruals models, we address concerns that performance differences may drive our results by replicating our tests using a performance-matched sample. Following Godsell et al. (2017), we select five match firms in the same two-digit SIC industry that are closest to the defense firm in ROA (with replacement). We describe the matched sample in Tables 1–4 of Online Appendix 1. We report results from replicating Tables 5–12 using this matched sample in Online Appendix 1. Overall, results are slightly stronger using the performance-matched sample (e.g., the test variable coefficient in the matched sample is −0.0130 [*t*-stat: −2.73] versus −0.0110 [*t*-stat: −2.36] in the full sample), and, across all of our tests, inferences drawn from the matched sample described in Online Appendix 1 corroborate inferences drawn from our full sample.

7.4.8. *Alternate sample #2: entropy-balanced sample*

Table 3 indicates that several defense firm characteristics are statistically different from those of non-defense firms. We repeat all of our analyses using an entropy-balanced sample to address concerns that differences in firm characteristics across treatment and control firms spuriously drive our empirical results. The recent accounting literature has extensively employed the entropy-balancing method to address selection concerns.[37] Entropy balancing weights control firm observations such that the mean, variance and skewness of the matching variables for control firms match the mean, variance and skewness of matching variables for treatment firms (Hainmueller, 2012; McMullin and Schonberger, 2020). The sum of the weights applied to control observations equals the number of treatment firm observations with each control observation receiving a weight between 0 and 1 (in comparison, all control observations in the main test pooled sample receive a weighting of 1). All treatment firm observations retain a weighting of 1. This procedure mitigates concerns regarding covariate balance in a manner similar to propensity score-matching but offers the incremental advantages of 1) retaining all data; 2) matching on the mean, variance and skewness (first, second and third moments) instead of just the mean; and 3) easier replicability given the many fewer research design decisions required when employing the entropy-balancing sampling procedure (McMullin and Schonberger, 2020; DeFond et al., 2016; Shipman et al., 2016).

[37] See, e.g., Ashraf et al. (2019); Chapman et al. (2019); Edwards et al. (2019); Ege et al. (2019); Gaver and Utke (2019); Glendening et al. (2019); Utke (2019).

We use the entropy-balancing procedure designed for Stata by Hainmueller and Xu (2013) to form the balanced sample. In implementing this procedure, we reweight control firm observations such that we achieve precise covariate balance for all but one control variable used in main tables. We exclude our idiosyncratic shock control variable from the entropy-balancing procedure because the procedure does not converge across all three moments (mean, variance and skewness) with its inclusion. We still include our idiosyncratic shock variable as a control variable throughout this set of analyses.[38]

Entropy-balanced control variables are listed in Online Appendix 2 Table 1. This table shows that control sample mean, variance and skewness of the chosen covariates match treatment firm mean, variance and skewness to the third decimal place after entropy balancing. We report results using the entropy-balanced sample in Online Appendix 2 Tables 5–12 The large majority of our inferences are corroborated by results from regression procedures using the entropy-balanced sample. While the economic magnitude of test variable coefficients is slightly lower across all models, the statistical significance of test variable coefficients is higher. The robustness of our inferences to an entropy-balanced sample with balanced firm characteristics addresses concerns that differences in firm characteristics across treatment and control firms spuriously drive our empirical results.

7.4.9. Alternate sample #3: defense firm-only sample

While we control for accrual determinants that distinguish defense firms from control firms, we address concerns that unobserved differences between defense firms and control firms drive our effect by replicating our tests after excluding all control firms. A defense firm-only sample mitigates concerns about innate differences between treatment and control firms because all firms in this sample are defense firms. Moreover, history suggests that the direct economic effects of variation in the threat of political costs (caused by, e.g., excess-profit taxes and price controls) affect all defense firms such that year fixed effects in this sample control for temporal variation in the threat of political costs for all defense firms. Controlling for the direct economic effects of variation in the threat of political costs improves our identification of discretionary accruals choices made by defense firms to orchestrate the appearance of lower profits in response to variation in local soldier fatalities. Consequently, this sample achieves covariate balance and permits well-identified tests of the political cost hypothesis.

We estimate the effect of variation in local soldier fatalities on defense firms' discretionary accruals using only 582 defense firm-years. We describe the defense firm-only sample in Tables 1–4 of Online Appendix 3. We report results from estimating Equation (1) using our defense firm-only sample in Tables 5–12 of Online Appendix 3. Our main inferences are corroborated using this sample (i.e., the test variable coefficient in the defense firm-only sample is -0.0112 [*t*-stat: -2.27] versus -0.0110 [*t*-stat: -2.36] in the full sample).

8. Conclusion

To test the political cost hypothesis, we use local soldier fatalities as a source of as-if-random variation in the threat of political costs for defense firms. Soldier fatalities vary the threat of political costs for defense firms because of the U.S. tradition of shared sacrifice during war. The U.S. tradition of shared sacrifice during war vulgarizes war profits amid soldier fatalities, and the public has supported government imposition of political costs on defense firms throughout U.S. history (Brandes, 1997). These political costs manifest as, for example, industry-wide excess-profit taxes and price controls. Consequently, local defense firms face an incentive to dull public opposition to war profits by orchestrating the appearance of lower profits when the threat of political costs increases. When the threat of political costs is high, the political cost hypothesis predicts that defense firms will orchestrate the appearance of lower profits through accounting choices.

In tests employing an exhaustive suite of accrual determinants and firm and industry $\times$ year fixed effects, we show that local defense firms record more income-decreasing accruals in response to a source of variation in the threat of political costs that randomly affects firms across time and place. We further find that the power of the political cost hypothesis to explain accounting choices varies predictably with state and firm characteristics. We conduct four placebo tests appropriate for our setting and fail to replicate our main results. Inferences from real actions-based earnings management tests support our main inferences. We find that our results are robust to alternative specifications, seven alternate discretionary accrual models, and three alternate samples. Overall, our causal evidence supports the political cost hypothesis and increases the credibility of non-causal inferences drawn from non-defense settings examined in the prior literature. Our evidence advances the political cost literature and sheds light on wartime earnings management by defense firms.

[38] In untabulated tests, we balance on the idiosyncratic shock variable but, to ensure convergence, balance only on mean and variance and not skewness. We find nearly identical coefficients on our test variable, *Local Solider Fatalities* $\times$ *Defense Firm*, leaving our inferences unchanged.

Appendix A. Variable Definitions

Variable Name	Definition	Source
Test Variables		
Local Soldier Fatalities	The number of local soldier fatalities in the twelve months leading up to firm *i*'s fiscal year-end in year *t*.	www.icasualties.org
Defense Firm	A variable equal to one for firms with year 2000 defense sales divided by total sales in excess of 5% and zero otherwise.	FPDS-NG
National Soldier Fatalities	The logarithm of the number of total soldier fatalities in the twelve months leading up to firm *i*'s fiscal year-end in year *t*.	www.icasualties.org
Accrual Model Control Variables		
Total Accruals	A variable equal to the change in current assets plus the change in current portion of long-term debt minus the sum of the change in cash, the change in current liabilities and depreciation [((ACT − lag1ACT) + (DLC − lag1DLC)) − ((CHE − lag1CHE) + (LCT − lag1LCT) + DP)]	CRSP/Compustat
1/Total Assets	1 divided by total assets (1/lag1AT)	CRSP/Compustat
ΔRevenue − ΔAccounts Receivable	The change in revenue minus the change in accounts receivable. ([(REVT − lag1REVT) − (RECT − lag1RECT)]/lag1AT)	CRSP/Compustat
PPE	Property plant and equipment. (PPENT/lag1AT)	CRSP/Compustat
Return on Assets (ROA)	Income before extraordinary items divided by lagged total assets. (IB/lag1AT)	CRSP/Compustat
Operating Cash Flows	Operating cash flows (OANCF)	CRSP/Compustat
Size	Logarithm of lagged revenues	CRSP/Compustat
Book Leverage	Long-term debt plus the current portion of long-term debt divided by lagged total assets. ([DLTT + DLC]/lag1AT)	CRSP/Compustat
Sales Growth	Growth is sales growth, defined as sales in *t* minus sales in $t-1$, divided by sales in $t-1$.	CRSP/Compustat
Change in Employees	Number of employees minus lagged number of employees divided by lagged number of employees. ([EMP − lag1EMP]/lag1EMP)	CRSP/Compustat
Std (sales)	The standard deviation of sales over the past three years, i.e., *t*, *t*-1, *t*-2. (std [SALE, lag1SALE, lag2SALE])	CRSP/Compustat
NOA_{t-1}	Net operating assets, calculated as the sum of shareholders equity and interest-bearing debt, minus cash assets, scaled by sales. ([SEQ + DLTT + DLC − CHE]/lag1SALE)	CRSP/Compustat
Market Return	Annual closing price minus lagged annual closing price divided by lagged annual closing price ([PRCC_F − lag1PRCC_F]/lag1PRCC_F)	CRSP/Compustat
Market-to-Book Ratio	Annual closing price × common shares outstanding divided by shareholders equity (or alternate variable) plus deferred taxes and investment tax credit minus preferred stock. ([PRCC_F × CSHO]/[(COALESCE [SEQ, CEQ + UPSTK, AT − LT] + TXDITC − COALESCE [PSTKRV, PSTKL, UPSTK])])	CRSP/Compustat
Economic Volatility and Idiosyncratic Risk Variables		
Loss	A variable equal to one if firm earnings before extraordinary items (IB) are below zero and zero otherwise.	CRSP/Compustat
Operating Cycle	Natural log of the firm's operating cycle measured in days, based on turnover in accounts receivable and receivable and inventory. Specifically, the firm's operating cycle is calculated as (180 × $[RECT_t + RECT_{t-1}]$ ÷ SALE) + ($[INV_t + INV_{t-1}]$ ÷ COGS).	CRSP/Compustat
Stock Volatility	Return volatility measured as the standard deviation of annual stock returns over the prior three fiscal years.	CRSP/Compustat
Standard Deviation of Earnings	Standard deviation of annual earnings before extraordinary items (IB) deflated by lagged total assets over the past three years.	CRSP/Compustat
Standard Deviation of Cash Flows	Standard deviation of annual cash flows deflated by lagged total assets over the past three years.	CRSP/Compustat
Industry Adjusted Cash Flows	Annual cash flows from operations minus the median cash flows from operations for all firms in the same industry (based on 2-digit SIC code) in the same quarter.	CRSP/Compustat
Below-Industry Cash Flow	An indicator variable equal to one if *Industry Adjusted Cash Flows* are below zero and zero otherwise.	CRSP/Compustat
Idiosyncratic Shock	We follow Owens et al. (2017) to generate the idiosyncratic shock variable, known as *IdioShock2* in the original paper. The variable is defined as the mean of the squared residuals from the following equation: $r_{i,T} = \alpha_{i,T} + \beta_1 r_{j,T} + \beta_2 r_{m,T} + \varepsilon_{i,T}$ Where $r_{i,T}$ is firm *i*'s monthly stock return, $r_{j,T}$ is the value-weighted monthly return for firm *i*'s industry excluding firm *i*'s return, and $r_{m,T}$ is the value-weighted market return. *T* indexes the 24 months in years *t* and *t*-1.	CRSP
Real Actions-Based Earnings Management Variables		
SG&A Expense	Sales, general and administration costs divided by lagged total assets and multiplied by −1	CRSP/Compustat
Production Costs	Production costs are measured as the sum of cost of goods sold plus the annual change in inventory divided by lagged assets, and an increase (decrease) in production costs has the effect of decreasing (increasing) cost of goods sold reported on the income statement and increasing (decreasing) net income.	CRSP/Compustat
Advertising Expense	Advertising expense divided by lagged total assets and multiplied by −1	CRSP/Compustat
R&D Expense	R&D expense divided by lagged total assets and multiplied by −1.	CRSP/Compustat

Appendix Table 1A
Testing for Path Dependence in Local Soldier Fatalities

	(1)	(2)	(3)
	Local Soldier Fatalities$_t$		
Local Soldier Fatalities$_{t-1}$	−0.0185	−0.0474	−0.0554
	(-0.23)	(-0.61)	(−0.66)
Local Soldier Fatalities$_{t-2}$		−0.0175	−0.0217
		(-0.22)	(−0.24)
Local Soldier Fatalities$_{t-3}$			−0.0855
			(−1.18)
Constant	2.464***	2.471***	2.588***
	(12.23)	(10.14)	(7.39)
R-Squared	0.503	0.533	0.560
Number of Observations	459	408	357
Standard Errors Clustered By:	State	State	State
State Fixed Effects:	Yes	Yes	Yes
Year Fixed Effects:	Yes	Yes	Yes

This table regresses current state-year fatalities on lagged state-year fatalities. State and year fixed effects are included in each column. The coefficients capture whether a deviation from the state mean of local soldier fatalities in one year predicts deviations from the state mean in soldier fatalities in a future year. Standard errors are clustered by state. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 1B
Testing for Relationship Between Military Expenditures and Soldier Fatalities

	(1)	(2)	(3)	(4)
	Procurement Revenues			
Local Soldier Fatalities × Defense Firm	**0.000372**	**0.000384**	**0.000249**	**−0.000258**
	(0.13)	**(0.13)**	**(0.09)**	**(-0.08)**
Defense Firm	−0.0195	−0.0208	−0.0189	
	(-0.62)	(-0.67)	(-0.60)	
Local Soldier Fatalities	0.0000402	0.0000370	0.000255	0.000287
	(0.33)	(0.30)	(1.29)	(1.40)
National Soldier Fatalities	0.000221	−0.000161		
	(0.60)	(-0.37)		
National Soldier Fatalities × Defense Firm	0.00456	0.00477	0.00448	0.00519
	(0.83)	(0.88)	(0.82)	(0.81)
Size		0.00000485	0.0000402	−0.000567
		(0.08)	(0.61)	(-1.54)
Book Leverage		0.000334	0.000561	0.00152
		(0.96)	(1.47)	(1.42)
Sales Growth		0.00591***	0.00608***	0.00690***
		(3.48)	(3.47)	(3.33)
Change in Employees		0.000352	0.000520	0.000336
		(0.48)	(0.71)	(0.37)
Market Return		0.0000700	0.0000154	0.0000130
		(0.60)	(0.14)	(0.10)
Market-to-Book Ratio		0.0000535	0.0000607	0.0000494
		(0.88)	(0.95)	(0.78)
Constant	−0.00133	0.000431	−0.00132**	0.00118
	(-0.62)	(0.17)	(-2.26)	(0.46)
R-Squared	0.0254	0.0360	0.0439	0.199
Number of Observations	16,749	16,749	16,749	16,749
Standard Errors Clustered By:	Firm	Firm	Firm	Firm
State Fixed Effects:	No	No	Yes	No

Appendix Table 1B (*continued*)

	(1)	(2)	(3)	(4)
	Procurement Revenues			
Firm Fixed Effects:	No	No	No	Yes
Industry × Year Fixed Effects:	No	No	Yes	Yes

This table reports results from regressions of DOD procurement sales on firm-level characteristics including Local Soldier Fatalities × Defense Firm. Local Soldier Fatalities is the per-capita number of Iraq and Afghanistan soldier fatalities by state and year of permanent state residence. Defense Firm is equal to one for firms that provide procured goods to the Department of Defense (DOD) and for which DOD revenues were in excess of 5% of total revenues. Other variables are defined in Appendix A. Standard errors are clustered by firm. t-statistics are presented underneath the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jacceco.2020.101316.

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